



# *The* ADVANCEMENT *of* SCIENCE: 1920



Addresses delivered at the 88th Annual Meeting of

THE BRITISH ASSOCIATION  
FOR THE ADVANCEMENT OF SCIENCE

CARDIFF, AUGUST 1920

LONDON

JOHN MURRAY, ALBEMARLE STREET

Office of the Association: BURLINGTON HOUSE, LONDON, W. 1

PRICE SIX SHILLINGS

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# British Association for the Advancement of Science.

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CARDIFF, 19:

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## ADDRESS<sup>Y</sup>

BY

WILLIAM A. HERDMAN, C.B.E., D.Sc., Sc.D., LL.D., F.R.S.,  
Professor of Oceanography in the University of Liverpool.

PRESIDENT.

### *Oceanography and the Sea-Fisheries.*

It has been customary, when occasion required, for the President to offer a brief tribute to the memory of distinguished members of the Association lost to Science during the preceding year. These, for the most part, have been men of advanced years and high reputation, who had completed their life-work and served well in their day the Association and the sciences which it represents. We have this year no such losses to record. But it seems fitting on the present occasion to pause for a moment and devote a grateful thought to that glorious band of fine young men of high promise in science who, in the years since our Australian meeting in 1914, gave, it may be, in brief days and months of sacrifice, greater service to humanity and the advance of civilisation than would have been possible in years of normal time and work. A few names stand out already known and highly honoured—Moseley, Jenkinson, Geoffrey Smith, Keith Lucas, Gregory, and more recently Leonard Doncaster—all grievous losses; but there are also others, younger members of our Association, who had not yet had opportunity for showing accomplished work, but who equally gave up all for a great ideal. I prefer to offer a collective rather than an individual tribute. Other young men of science will arise and carry on their work—but the gap in our ranks remains. Let their successors remember that it serves as a reminder of a great example and of high endeavour worthy of our gratitude and of permanent record in the annals of Science.

At the last Cardiff Meeting of the British Association in 1891 you had as your President the eminent astronomer Sir William Huggins, who discoursed upon the then recent discoveries of the spectroscope in relation to the chemical nature, density, temperature, pressure and even the motions of the stars. From the sky to the sea is a long drop; but the sciences of both have this in common that they deal with



fundamental principles and with vast numbers. Over three hundred years ago Spenser in the 'Faerie Queene' compared 'the seas abundant progeny' with 'the starres on hy,' and recent investigations show that a litre of sea-water may contain more than a hundred times as many living organisms as there are stars visible to the eye on a clear night.

During the past quarter of a century great advances have been made in the science of the sea, and the aspects and prospects of sea-fisheries research have undergone changes which encourage the hope that a combination of the work now carried on by hydrographers and biologists in most civilised countries on fundamental problems of the ocean may result in a more rational exploitation and administration of the fishing industries.

And yet even at your former Cardiff Meeting thirty years ago there were at least three papers of oceanographic interest—one by Professor Osborne Reynolds on the action of waves and currents, another by Dr. H. R. Mill on seasonal variation in the temperature of lochs and estuaries, and the third by our Honorary Local Secretary for the present meeting, Dr. Evans Hoyle, on a deep-sea tow-net capable of being opened and closed under water by the electric current.

It was a notable meeting in several other respects, of which I shall merely mention two. In Section A, Sir Oliver Lodge gave the historic address in which he expounded the urgent need, in the interests of both science and the industries, of a national institution for the promotion of physical research on a large scale. Lodge's pregnant idea put forward at this Cardiff Meeting, supported and still further elaborated by Sir Douglas Galton as President of the Association at Ipswich, has since borne notable fruit in the establishment and rapid development of the National Physical Laboratory. The other outstanding event of that meeting is that you then appointed a committee of eminent geologists and naturalists to consider a project for boring through a coral reef, and that led during following years to the successive expeditions to the atoll of Funafuti in the Central Pacific, the results of which, reported upon eventually by the Royal Society, were of great interest alike to geologists, biologists, and oceanographers.

Dr. Huggins, on taking the Chair in 1891, remarked that it was over thirty years since the Association had honoured Astronomy in the selection of its President. It might be said that the case of Oceanography is harder, as the Association has never had an Oceanographer as President—and the Association might well reply 'Because until very recent years there has been no Oceanographer to have.' If Astronomy is the oldest of the sciences, Oceanography is probably the youngest. Depending as it does upon the methods and results of other sciences, it was not until our knowledge of Physics, Chemistry, and Biology were

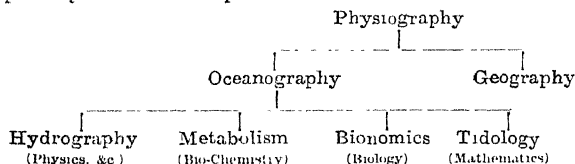
relatively far advanced that it became possible to apply that knowledge to the investigation and explanation of the phenomena of the ocean. No one man has done more to apply such knowledge derived from various other subjects and to organise the results as a definite branch of science than the late Sir John Murray, who may therefore be regarded as the founder of modern Oceanography.

It is, to me, a matter of regret that Sir John Murray was never President of the British Association. I am revealing no secret when I tell you that he might have been. On more than one occasion he was invited by the Council to accept nomination and he declined for reasons that were good and commanded our respect. He felt that the necessary duties of this post would interfere with what he regarded as his primary life-work—oceanographical explorations already planned, and the last of which he actually carried out in the North Atlantic in 1912, when over seventy years of age, in the Norwegian steamer *Michael Sars* along with his friend Dr. Johan Hjort.

Anyone considering the subject-matter of this new science must be struck by its wide range, overlapping as it does the borderlands of several other sciences and making use of their methods and facts in the solution of its problems. It is not only world-wide in its scope but extends beyond our globe and includes astronomical data in their relation to tidal and certain other oceanographical phenomena. No man in his work, or even thought, can attempt to cover the whole ground—although Sir John Murray, in his remarkably comprehensive 'Summary' volumes of the *Challenger* Expedition and other writings, went far towards doing so. He, in his combination of physicist, chemist, geologist and biologist, was the nearest approach we have had to an all-round Oceanographer. The International Research Council probably acted wisely at the recent Brussels Conference in recommending the institution of two International Sections in our subject, the one of physical and the other of biological Oceanography—although the two overlap and are so interdependent that no investigator on the one side can afford to neglect the other.<sup>1</sup>

On the present occasion I must restrict myself almost wholly to the latter division of the subject, and be content, after brief reference to the

<sup>1</sup> The following classification of the primary divisions of the subject may possibly be found acceptable :—



founders and pioneers of our science, to outline a few of those investigations and problems which have appeared to me to be of fundamental importance, of economic value, or of general interest.

Although the name Oceanography was only given to this branch of science by Sir John Murray in 1880, and although according to that veteran oceanographer Mr. J. Y. Buchanan, the last surviving member of the civilian staff of the *Challenger*, the science of Oceanography was born at sea on February 15, 1873,<sup>2</sup> when, at the first official dredging station of the expedition, to the westward of Teneriffe, at 1525 fathoms, everything that came up in the dredge was new and led to fundamental discoveries as to the deposits forming on the floor of the ocean, still it may be claimed that the foundations of the science were laid by various explorers of the ocean at much earlier dates. Aristotle, who took all knowledge for his province, was an early oceanographer on the shores of Asia Minor. When Pytheas passed between the pillars of Hercules into the unknown Atlantic and penetrated to British seas in the fourth century B.C., and brought back reports of Ultima Thule and of a sea to the North thick and sluggish like a jelly-fish, he may have been recording an early planktonic observation. But passing over all such and many other early records of phenomena of the sea, we come to surer ground in claiming, as founders of Oceanography, Count Marsili, an early investigator of the Mediterranean, and that truly scientific navigator Captain James Cook, who sailed to the South Pacific on a Transit of Venus expedition in 1769 with Sir Joseph Banks as naturalist, and by subsequently circumnavigating the South Sea about latitude 60° finally disproved the existence of a great southern continent; and Sir James Clerk Ross, who, with Sir Joseph Hooker as naturalist, first dredged the Antarctic in 1840.

The use of the naturalist's dredge (introduced by O. F. Muller, the Dane, in 1799) for exploring the sea-bottom was brought into prominence almost simultaneously in several countries of North-West Europe —by Henri Milne-Edwards in France in 1830, Michael Sars in Norway in 1835, and our own Edward Forbes about 1832.

The last-mentioned genial and many-sided genius was a notable figure in several sections of the British Association from about 1836 onwards, and may fairly be claimed as a pioneer of Oceanography. In 1839 he and his friend the anatomist, John Goodsir, were dredging

<sup>2</sup> Others might put the date later. Significant publications are Sir John Murray's Summary Volumes of the *Challenger* (1895), the inauguration of the 'Musée Océanographique' at Monaco in 1910, the foundation of the 'Institut Océanographique' at Paris in 1906 (see the Prince of Monaco's letter to the Minister of Public Instruction), and Sir John Murray's little book: 'The Ocean' (1913), where the superiority of the term Oceanography to Thalassography (used by Alexander Agassiz) is discussed.

in the Shetland seas, with results which Forbes made known to the meeting of the British Association at Birmingham that summer, with such good effect that a 'Dredging Committee'<sup>3</sup> of the Association was formed to continue the good work. Valuable reports on the discoveries of that Committee appear in our volumes at intervals during the subsequent twenty-five years.

It has happened over and over again in history that the British Association, by means of one of its research committees, has led the way in some important new research or development of science and has shown the Government or an industry what wants doing and how it can be done. We may fairly claim that the British Association has inspired and fostered that exploration of British seas which through marine biological investigations and deep-sea expeditions has led on to modern Oceanography. Edward Forbes and the British Association Dredging Committee, Wyville Thomson, Carpenter, Gwyn Jeffreys, Norman and other naturalists of the pre-*Challenger* days—all these men in the quarter-century from 1840 onwards worked under research committees of the British Association, bringing their results before successive meetings; and some of our older volumes enshrine classic reports on dredging by Forbes, McAndrew, Norman, Brady, Alder, and other notable naturalists of that day. These local researches paved the way for the *Challenger* and other national deep-sea expeditions. Here, as in other cases, it required private enterprise to precede and stimulate Government action.

It is probable that Forbes and his fellow-workers on this 'Dredging Committee' in their marine explorations did not fully realise that they were opening up a most comprehensive and important department of knowledge. But it is also true that in all his expeditions—in the British seas from the Channel Islands to the Shetlands, in Norway, in the Mediterranean as far as the Ægean Sea—his broad outlook on the problems of nature was that of the modern oceanographer, and he was the spiritual ancestor of men like Sir Wyville Thomson of the *Challenger* Expedition and Sir John Murray, whose accidental death a few years ago, while still in the midst of active work, was a grievous loss to this new and rapidly advancing science of the sea.

Forbes in these marine investigations worked at border-line problems, dealing for example with the relations of Geology to Zoology.

<sup>3</sup> 'For researches with the dredge, with a view to the investigation of the marine zoology of Great Britain, the illustration of the geographical distribution of marine animals, and the more accurate determination of the fossils of the pleistocene period: under the superintendence of Mr. Gray, Mr. Forbes, Mr. Goodsir, Mr. Patterson, Mr. Thompson of Belfast, Mr. Ball of Dublin, Dr. George Johnston, Mr. Smith of Jordan Hill, and Mr. A. Strickland, £60.' Report for 1839, p. xxvi.

and the effect of the past history of the land and sea upon the distribution of plants and animals at the present day, and in these respects he was an early oceanographer. For the essence of that new subject is that it also investigates border-line problems and is based upon and makes use of all the older fundamental sciences—Physics, Chemistry and Biology—and shows for example how variations in the great ocean currents may account for the movements and abundance of the migratory fishes, and how periodic changes in the physico-chemical characters of the sea, such as variations in the hydrogen-ion and hydroxyl-ion concentration, are correlated with the distribution at the different seasons of the all-important microscopic organisms that render our oceanic waters as prolific a source of food as the pastures of the land.

Another pioneer of the nineteenth century who, I sometimes think, has not yet received sufficient credit for his foresight and initiative, is Sir Wyville Thomson, whose name ought to go down through the ages as the leader of the scientific staff on the famous *Challenger* Deep-Sea Exploring Expedition. It is due chiefly to him and to his friend Dr. W. B. Carpenter that the British Government, through the influence of the Royal Society, was induced to place at the disposal of a committee of scientific experts first the small surveying steamer *Lightning* in 1868, and then the more efficient steamer *Porcupine* in the two succeeding years, for the purpose of exploring the deep water of the Atlantic from the Faroes in the North to Gibraltar and beyond in the South, in the course of which expeditions they got successful hauls from the then unprecedented depth of 2435 fathoms, nearly three statute miles.

It will be remembered that Edward Forbes, from his observations in the Mediterranean (an abnormal sea in some respects), regarded depths of over 300 fathoms as an azoic zone. It was the work of Wyville Thomson and his colleagues Carpenter and Gwyn Jeffreys on these successive dredging expeditions to prove conclusively what was beginning to be suspected by naturalists, that there is no azoic zone in the sea, but that abundant life belonging to many groups of animals extends down to the greatest depths of from four to five thousand fathoms—nearly six statute miles from the surface.

These pioneering expeditions in the *Lightning* and *Porcupine*—the results of which are not even yet fully made known to science—were epoch-making, inasmuch as they not only opened up this new region to the systematic marine biologist, but gave glimpses of world-wide problems in connection with the physics, the chemistry and the biology of the sea which are only now being adequately investigated by the modern oceanographer. These results, which aroused intense interest amongst the leading scientific men of the time, were so rapidly surpassed and overshadowed by the still greater achievements of the

*Challenger* and other national exploring expeditions that followed in the 'seventies and 'eighties of last century, that there is some danger of their real importance being lost sight of; but it ought never to be forgotten that they first demonstrated the abundance of life of a varied nature in depths formerly supposed to be azoic, and, moreover, that some of the new deep-sea animals obtained were related to extinct forms belonging to the Jurassic, Cretaceous and Tertiary periods.

It is interesting to recall that our Association played its part in promoting the movement that led to the *Challenger* Expedition. Our General Committee at the Edinburgh Meeting of 1871 recommended that the President and Council be authorised to co-operate with the Royal Society in promoting 'a Circumnavigation Expedition, specially fitted out to carry the Physical and Biological Exploration of the Deep Sea into all the Great Oceanic Areas'; and our Council subsequently appointed a committee consisting of Dr. Carpenter, Professor Huxley and others to co-operate with the Royal Society in carrying out these objects.

It has been said that the *Challenger* Expedition will rank in history with the voyages of Vasco da Gama, Columbus, Magellan and Cook. Like these it added new regions of the globe to our knowledge, and the wide expanses thus opened up for the first time, the floors of the oceans, though less accessible, are vaster than the discoveries of any previous exploration.

Sir Wyville Thomson, although leader of the expedition, did not live to see the completed results, and Sir John Murray will be remembered in the history of science as the *Challenger* naturalist who brought to a successful issue the investigation of the enormous collections and the publication of the scientific results of that memorable voyage: these two Scots share the honour of having guided the destinies of what is still the greatest oceanographic exploration of all times.

In addition to taking his part in the general work of the expedition, Murray devoted special attention to three subjects of primary importance in the science of the sea, viz.: (1) the plankton or floating life of the oceans, (2) the deposits forming on the sea bottoms, and (3) the origin and mode of formation of coral reefs and islands. It was characteristic of his broad and synthetic outlook on nature that, in place of working at the speciology and anatomy of some group of organisms, however novel, interesting and attractive to the naturalist the deep-sea organisms might seem to be, he took up wide-reaching general problems with economic and geological as well as biological applications.

Each of the three main lines of investigation—deposits, plankton and coral reefs—which Murray undertook on board the *Challenger* has been most fruitful of results both in his own hands and those of

others His plankton work has led on to those modern planktonic researches which are closely bound up with the scientific investigation of our sea-fisheries.

His work on the deposits accumulating on the floor of the ocean resulted, after years of study in the laboratory as well as in the field, in collaboration with the Abbé Renard of the Brussels Museum, afterwards Professor at Ghent, in the production of the monumental 'Deep-Sea Deposits' volume, one of the *Challenger* Reports, which first revealed to the scientific world the detailed nature and distribution of the varied submarine deposits of the globe and their relation to the rocks forming the crust of the earth.

These studies led, moreover, to one of the romances of science which deeply influenced Murray's future life and work. In accumulating material from all parts of the world and all deep-sea exploring expeditions for comparison with the *Challenger* series, some ten years later, Murray found that a sample of rock from Christmas Island in the Indian Ocean, which had been sent to him by Commander (now Admiral) Aldrich, of H.M.S. *Egeria*, was composed of a valuable phosphatic material. This discovery in Murray's hands gave rise to a profitable commercial undertaking, and he was able to show that some years ago the British Treasury had already received in royalties and taxes from the island considerably more than the total cost of the *Challenger* Expedition.

That first British circumnavigating expedition on the *Challenger* was followed by other national expeditions (the American *Tuscarora* and *Albatross*, the French *Travailleur*, the German *Gauss*, *National*, and *Valdivia*, the Italian *Vettor Pisani*, the Dutch *Siboga*, the Danish *Thor* and others) and by almost equally celebrated and important work by unofficial oceanographers such as Alexander Agassiz, Sir John Murray with Dr. Hjort in the *Michael Sars*, and the Prince of Monaco in his magnificent ocean-going yacht, and by much other good work by many investigators in smaller and humbler vessels. One of these supplementary expeditions I must refer to briefly because of its connection with sea-fisheries. The *Triton*, under Tizard and Murray, in 1882, while exploring the cold and warm areas of the Faroe Channel separated by the Wyville-Thomson ridge, incidentally discovered the famous Dubh-Artach fishing-grounds, which have been worked by British trawlers ever since.

Notwithstanding all this activity during the last forty years since Oceanography became a science, much has still to be investigated in all seas in all branches of the subject. On pursuing any line of investigation one very soon comes up against a wall of the unknown or a maze of controversy. Peculiar difficulties surround the subject. The

matters investigated are often remote and almost inaccessible. Unknown factors may enter into every problem. The samples required may be at the other end of a rope or a wire eight or ten miles long, and the oceanographer may have to grope for them literally in the dark and under other difficult conditions which make it uncertain whether his samples when obtained are adequate and representative, and whether they have undergone any change since leaving their natural environment. It is not surprising then that in the progress of knowledge mistakes have been made and corrected, that views have been held on what seemed good scientific grounds which later on were proved to be erroneous. For example, Edward Forbes, in his division of life in the sea into zones, on what then seemed to be sufficiently good observations in the Ægean, but which we now know to be exceptional, placed the limit of life at 300 fathoms, while Wyville Thomson and his fellow-workers on the *Porcupine* and the *Challenger* showed that there is no azoic zone even in the great abysses.

Or, again, take the celebrated myth of 'Bathybius.' In the 'sixties of last century samples of Atlantic mud, taken when surveying the bottom for the first telegraph cables and preserved in alcohol, were found when examined by Huxley, Haeckel and others to contain what seemed to be an exceedingly primitive protoplasmic organism, which was supposed on good evidence to be widely extended over the floor of the ocean. The discovery of this Bathybius was said to solve the problem of how the deep-sea animals were nourished in the absence of seaweeds. Here was a widespread protoplasmic meadow upon which other organisms could graze. Belief in Bathybius seemed to be confirmed and established by Wyville Thomson's results in the *Porcupine* Expedition of 1869, but was exploded by the naturalists on the *Challenger* some five years later. Buchanan in his recently published 'Accounts Rendered' tells us how he and his colleague Murray were keenly on the look-out for hours at a time on all possible occasions for traces of this organism, and how they finally proved, in the spring of 1875 on the voyage between Hong-Kong and Yokohama, that the all-pervading substance like coagulated mucus was an amorphous precipitate of sulphate of lime thrown down from the sea-water in the mud on the addition of a certain proportion of alcohol. He wrote to this effect from Japan to Professor Crum Brown, and it is in evidence that after receiving this letter Crum Brown interested his friends in Edinburgh by showing them how to make Bathybius in the chemical laboratory. Huxley at the Sheffield Meeting of the British Association in 1879 handsomely admitted that he had been mistaken, and it is said that he characterised Bathybius as 'not having fulfilled the promise of its youth.' Will any of our present oceanographic beliefs



share the fate of Bathybius in the future? Some may, but even if they do they may well have been useful steps in the progress of science. Although like Bathybius they may not have fulfilled the promise of their youth, yet, we may add, they will not have lived in the minds of man in vain.

Many of the phenomena we encounter in oceanographic investigations are so complex, are or may be affected by so many diverse factors, that it is difficult, if indeed possible, to be sure that we are unravelling them aright and that we see the real causes of what we observe.

Some few things we know approximately—nothing completely. We know that the greatest depths of the ocean, about six miles, are a little greater than the highest mountains on land, and Sir John Murray has calculated that if all the land were washed down into the sea the whole globe would be covered by an ocean averaging about two miles in depth.<sup>4</sup> We know the distribution of temperatures and salinities over a great part of the surface and a good deal of the bottom of the oceans, and some of the more important oceanic currents have been charted and their periodic variations, such as those of the Gulf Stream, are being studied. We know a good deal about the organisms floating or swimming in the surface waters (the epi-plankton), and also those brought up by our dredges and trawls from the bottom in many parts of the world—although every expedition still makes large additions to knowledge. The region that is least known to us, both in its physical conditions and also its inhabitants, is the vast zone of intermediate waters lying between the upper few hundred fathoms and the bottom. That is the region that Alexander Agassiz from his observations with closing tow-nets on the *Blake* Expedition supposed to be destitute of life, or at least, as modified by his later observations on the *Albatross*, to be relatively destitute compared with the surface and the bottom, in opposition to the contention of Murray and other oceanographers that an abundant meso-plankton was present, and that certain groups of animals, such as the Challengerida and some kinds of Medusæ, were characteristic of these deeper zones. I believe that, as sometimes happens in scientific controversies, both sides were right up to a point, and both could support their views upon observations from particular regions of the ocean under certain circumstances.

But much still remains unknown or only imperfectly known even in matters that have long been studied and where practical applications

<sup>4</sup> It was possibly in such a former world-wide ocean of ionised water that according to the recent speculations of A. H. Church (*Thalassiphyta*, 1919) the first living organisms were evolved to become later the floating unicellular plants of the primitive plankton.

of great value are obtained—such as the investigation and prediction of tidal phenomena. We are now told that theories require re-investigation and that published tables are not sufficiently accurate. To take another practical application of oceanographic work, the ultimate causes of variations in the abundance, in the sizes, in the movements and in the qualities of the fishes of our coastal industries are still to seek, and notwithstanding volumes of investigation and a still greater volume of discussion, no man who knows anything of the matter is satisfied with our present knowledge of even the best-known and economically most important of our fishes such as the Herring, the Cod, the Plaice and the Salmon.

Take the case of our common fresh-water eel as an example of how little we know and at the same time of how much has been discovered. All the eels of our streams and lakes of N.-W. Europe live and feed and grow under our eyes without reproducing their kind—no spawning eel has ever been seen. After living for years in immaturity, at last near the end of their lives the large male and female yellow eels undergo a change in appearance and in nature. They acquire a silvery colour and their eyes enlarge, and in this bridal attire they commence the long journey which ends in maturity, reproduction and death. From all the fresh waters they migrate in the autumn to the coast, from the inshore seas to the open ocean and still westward and south to the mid-Atlantic and we know not how much further—for the exact locality and manner of spawning has still to be discovered. The youngest known stages of the *Leptocephalus*, the larval stage of eels, have been found by the Dane, Dr. Johannes Schmidt, to the west of the Azores where the water is over 2000 fathoms in depth. These were about one-third of an inch in length and were probably not long hatched. I cannot now refer to all the able investigators—Grassi, Hjort and others—who have discovered and traced the stages of growth of the *Leptocephalus* and its metamorphosis into the 'elvers' or young eels which are carried by the North Atlantic drift back to the coasts of Europe and ascend our rivers in spring in countless myriads; but no man has been more indefatigable and successful in the quest than Dr. Schmidt, who in the various expeditions of the Danish Investigation Steamer *Thor* from 1904 onwards found successively younger and younger stages, and who is during the present summer engaged in a traverse of the Atlantic to the West Indies in the hope of finding the missing link in the chain, the actual spawning fresh-water eel in the intermediate waters somewhere above the abysses of the open ocean.<sup>5</sup>

<sup>5</sup> According to Schmidt's results the European fresh-water eel, in order to be able to propagate, requires a depth of at least 500 fathoms, a salinity of more than 35.20 per mille and a temperature of more than 7° C. in the required depth.

Again, take the case of an interesting oceanographic observation which, if established, may be found to explain the variations in time and amount of important fisheries. Otto Pettersson in 1910 discovered by his observations in the Gullmar Fjord the presence of periodic submarine waves of deeper saltier water in the Kattegat and the fjords of the west coast of Sweden, which draw in with them from the Jutland banks vast shoals of the herrings which congregate there in autumn. The deeper layer consists of 'bankwater' of salinity 32 to 34 per thousand, and as this rolls in along the bottom as a series of huge undulations it forces out the overlying fresher water, and so the herrings living in the bankwater outside are sucked into the Kattegat and neighbouring fjords and give rise to important local fisheries. Pettersson connects the crests of the submarine waves with the phases of the moon. Two great waves of saltier water which reached up to the surface took place in November 1910, one near the time of full moon and the other about new moon, and the latter was at the time when the shoals of herring appeared inshore and provided a profitable fishery. The coincidence of the oceanic phenomena with the lunar phases is not, however, very exact, and doubts have been expressed as to the connection; but if established, and even if found to be due not to the moon but to prevalent winds or the influence of ocean currents, this would be a case of the migration of fishes depending upon mechanical causes, while in other cases it is known that migrations are due to spawning needs or for the purpose of feeding, as in the case of the cod and the herring in the west and north of Norway and in the Barents Sea.

Then, turning to a very fundamental matter of purely scientific investigation, we do not know with any certainty what causes the great and all-important seasonal variations in the plankton (or floating minute life of the sea) as seen, for example, in our own home seas, where there is a sudden awakening of microscopic plant life, the Diatoms, in early spring when the water is at its coldest. In the course of a few days the upper layers of the sea may become so filled with organisms that a small silk net towed for a few minutes may capture hundreds of millions of individuals. And these myriads of microscopic forms, after persisting for a few weeks, may disappear as suddenly as they came, to be followed by swarms of Copepoda and many other kinds of minute animals, and these again may give place in the autumn to a second maximum of Diatoms or of the closely related Peridinales. Of course there are theories as to all these more or less periodic changes in the plankton, such as Liebig's 'law of the minimum,' which limits the production of an organism by the amount of that necessity of existence which is present in least quantity, it may be nitrogen or silicon or

phosphorus. According to Raben it is the accumulation of silicic acid in the sea-water that determines the great increase of Diatoms in spring and again in autumn. Some writers have considered these variations in the plankton to be caused largely by changes in temperature supplemented, according to Ostwald, by the resulting changes in the viscosity of the water; but Murray and others are more probably correct in attributing the spring development of phyto-plankton to the increasing power of the sunlight and its value in photosynthesis.

Let us take next the fact—if it be a fact—that the genial warm waters of the tropics support a less abundant plankton than the cold polar seas. The statement has been made and supported by some investigators and disputed by others, both on a certain amount of evidence. This is possibly a case like some other scientific controversies where both sides are partly in the right, or right under certain conditions. At any rate there are marked exceptions to the generalisation. The German Plankton Expedition in 1889 showed in its results that much larger hauls of plankton per unit volume of water were obtained in the temperate North and South Atlantic than in the tropics between, and that the warm Sargasso Sea had a remarkably scanty microflora. Other investigators have since reported more or less similar results. Lohmann found the Mediterranean plankton to be less abundant than that of the Baltic, gatherings brought back from tropical seas are frequently very scanty, and enormous hauls on the other hand have been recorded from Arctic and Antarctic seas. There is no doubt about the large gatherings obtained in northern waters. I have myself in a few minutes' haul of a small horizontal net in the North of Norway collected a mass of the large Copepod *Calanus finmarchicus* sufficient to be cooked and eaten like potted shrimps by half a dozen of the yacht's company, and I have obtained similar large hauls in the cold Labrador current near Newfoundland. On the other hand, Kofoid and Alexander Agassiz have recorded large hauls of plankton in the Humboldt current off the west coast of America, and during the *Challenger* Expedition some of the largest quantities of plankton were found in the equatorial Pacific. Moreover, it is common knowledge that on occasions vast swarms of some planktonic organism may be seen in tropical waters. The yellow alga *Trichodesmium*, which is said to have given its name to the Red Sea and has been familiarly known as 'sea-sawdust' since the days of Cook's first voyage,<sup>6</sup> may cover the entire surface over considerable areas of the Indian and South Atlantic Oceans; and some pelagic animals such as Salpæ, Medusæ and Ctenophores are also commonly present in abundance in the tropics. Then, again, American

<sup>6</sup> See Journal of Sir Joseph Banks. This and other swarms were also noticed by Darwin during the voyage of the 'Beagle.'

biologists<sup>7</sup> have pointed out that the warm waters of the West Indies and Florida may be noted for the richness of their floating life for periods of years, while at other times the pelagic organisms become rare and the region is almost a desert sea.

It is probable, on the whole, that the distribution and variations of oceanic currents have more than latitude or temperature alone to do with any observed scantiness of tropical plankton. These mighty rivers of the ocean in places teem with animal and plant life, and may sweep abundance of food from one region to another in the open sea.

But even if it be a fact that there is this alleged deficiency in tropical plankton there is by no means agreement as to the cause thereof. Brandt first attributed the poverty of the plankton in the tropics to the destruction of nitrates in the sea as a result of the greater intensity of the metabolism of denitrifying bacteria in the warmer water; and various other writers since then have more or less agreed that the presence of these denitrifying bacteria, by keeping down to a minimum the nitrogen concentration in tropical waters, may account for the relative scarcity of the phyto-plankton, and consequently of the zoo-plankton, that has been observed. But Gran, Nathansohn, Murray, Hjort and others have shown that such bacteria are rare or absent in the open sea, that their action must be negligible, and that Brandt's hypothesis is untenable. It seems clear, moreover, that the plankton does not vary directly with the temperature of the water. Furthermore, Nathansohn has shown the influence of the vertical circulation in the water upon the nourishment of the phyto-plankton—by rising currents bringing up necessary nutrient materials, and especially carbon dioxide from the bottom layers; and also possibly by conveying the products of the drainage of tropical lands to more polar seas so as to maintain the more abundant life in the colder water.

Pütter's view is that the increased metabolism in the warmer water causes all the available food materials to be rapidly used up, and so puts a check to the reproduction of the plankton.

According to van t'Hoff's law in Chemistry, the rate at which a reaction takes place is increased by raising the temperature, and this probably holds good for all bio-chemical phenomena, and therefore for the metabolism of animals and plants in the sea. This has been verified experimentally in some cases by J. Loeb. The contrast between the plankton of Arctic and Antarctic zones, consisting of large numbers of small Crustaceans belonging to comparatively few species, and that of tropical waters, containing a great many more species generally of smaller size and fewer in number of individuals, is to be

<sup>7</sup> A. Agassiz, A. G. Mayer, and H. B. Bigelow.

accounted for, according to Sir John Murray and others, by the rate of metabolism in the organisms. The assemblages captured in cold polar waters are of different ages and stages, young and adults of several generations occurring together in profusion,<sup>8</sup> and it is supposed that the adults 'may be ten, twenty or more years of age.' At the low temperature the action of putrefactive bacteria and of enzymes is very slow or in abeyance, and the vital actions of the Crustacea take place more slowly and the individual lives are longer. On the other hand, in the warmer waters of the tropics the action of the bacteria is more rapid, metabolism in general is more active, and the various stages in the life-history are passed through more rapidly, so that the smaller organisms of equatorial seas probably only live for days or weeks in place of years.

This explanation may account also for the much greater quantity of living organisms which has been found so often on the sea floor in polar waters. It is a curious fact that the development of the polar marine animals is in general 'direct' without larval pelagic stages, the result being that the young settle down on the floor of the ocean in the neighbourhood of the parent forms, so that there come to be enormous congregations of the same kind of animal within a limited area, and the dredge will in a particular haul come up filled with hundreds, it may be, of an Echinoderm, a Sponge, a Crustacean, a Brachiopod, or an Ascidian; whereas in warmer seas the young pass through a pelagic stage and so become more widely distributed over the floor of the ocean. The *Challenger* Expedition found in the Antarctic certain Echinoderms, for example, which had young in various stages of development attached to some part of the body of the parents, whereas in temperate or tropical regions the same class of animals set free their eggs and the development proceeds in the open water quite independently of, and it may be far distant from, the parent.

Another characteristic result of the difference in temperature is that the secretion of carbonate of lime in the form of shells and skeletons proceeds more rapidly in warm than in cold water. The massive shells of molluscs, the vast deposits of carbonate of lime formed by corals and by calcareous seaweeds, are characteristic of the tropics; whereas in polar seas, while the animals may be large, they are for the most part soft-bodied and destitute of calcareous secretions. The calcareous pelagic Foraminifera are characteristic of tropical and sub-tropical plankton, and few, if any, are found in polar waters. Globigerina

<sup>8</sup> Whether, however, the low temperature may not also retard reproduction is worthy of consideration.

ooze, a calcareous deposit, is abundant in equatorial seas, while in the Antarctic the characteristic deposit is siliceous Diatomaceous ooze.

The part played by bacteria in the metabolism of the sea is very important and probably of wide-reaching effect, but we still know very little about it. A most promising young Cambridge biologist, the late Mr. G. Harold Drew, now unfortunately lost to science, had already done notable work at Jamaica and at Tortugas, Florida, on the effects produced by a bacillus which is found in the surface waters of these shallow tropical seas and in the mud at the bottom; and which denitrifies nitrates and nitrites, giving off free nitrogen. He found that this *Bacillus calcis* also caused the precipitation of soluble calcium salts in the form of calcium carbonate ('drewite') on a large scale, in the warm shallow waters. Drew's observations tend to show that the great calcareous deposits of Florida and the Bahamas previously known as 'coral muds' are not, as was supposed by Murray and others, derived from broken-up corals, shells, nullipores, &c, but are minute particles of carbonate of lime which have been precipitated by the action of these bacteria.<sup>9</sup>

The bearing of these observations upon the formation of oolitic limestones and the fine-grained unfossiliferous Lower Palæozoic limestones of New York State, recently studied in this connection by R. M. Field,<sup>10</sup> must be of peculiar interest to geologists, and forms a notable instance of the annectant character of Oceanography, bringing the metabolism of living organisms in the modern sea into relation with palæozoic rocks.

The work of marine biologists on the plankton has been in the main *qualitative*, the identification of species, the observation of structure, and the tracing of life-histories. The oceanographer adds to that the *quantitative* aspect when he attempts to estimate numbers and masses per unit volume of water or of area. Let me lay before you a few thoughts in regard to some such attempts, mainly for the purpose of showing the difficulties of the investigation. Modern quantitative methods owe their origin to the ingenious and laborious work of Victor Hensen, followed by Brandt, Apstein, Lohmann, and others of the Kiel school of quantitative planktologists. We may take their well-known estimations of fish eggs in the North Sea as an example of the method.

The floating eggs and embryos of our more important food fishes may occur in quantities in the plankton during certain months in spring, and Hensen and Apstein have made some notable calculations

<sup>9</sup> *Journ. Mar. Biol. Assoc.*, October 1911.

<sup>10</sup> Carnegie Institute of Washington. Year Book for 1919, p. 197.

based on the occurrence of these in certain hauls taken at intervals across the North Sea, which led them to the conclusion that, taking six of our most abundant fish, such as the cod and some of the flat fish, the eggs present were probably produced by about 1200 million spawners, enabling them to calculate that the total fish population of the North Sea (of these six species), at that time (spring of 1895), amounted to about 10,000 millions. Further calculations led them to the result that the fishermen's catch of these fishes amounted to about one-quarter of the total population. Now all this is not only of scientific interest, but also of great practical importance if we could be sure that the samples upon which the calculations are based were adequate and representative, but it will be noted that these samples only represent one square metre in 3,465,968,354. Hensen's statement, repeated in various works in slightly differing words, is to the effect that, using a net of which the constants are known hauled vertically through a column of water from a certain depth to the surface, he can calculate the volume of water filtered by the net and so estimate the quantity of plankton under each square metre of the surface; and his whole results depend upon the assumption, which he considers justified, that the plankton is evenly distributed over large areas of water which are under similar conditions. In these calculations in regard to the fish eggs he takes the whole of the North Sea as being an area under similar conditions, but we have known since the days of P. T. Cleve and from the observations of Hensen's own colleagues that this is not the case, and they have published chart-diagrams showing that at least three different kinds of water under different conditions are found in the North Sea, and that at least five different planktonic areas may be encountered in making a traverse from Germany to the British Isles. If the argument be used that wherever the plankton is found to vary there the conditions cannot be uniform, then few areas of the ocean of any considerable size remain as cases suitable for population-computation from random samples. It may be doubted whether even the Sargasso Sea, which is an area of more than usually uniform character, has a sufficiently evenly distributed plankton to be treated by Hensen's method of estimation of the population.

In the German Plankton Expedition of 1889 Schütt reports that in the Sargasso Sea, with its relatively high temperature, the twenty-four catches obtained were uniformly small in quantity. His analysis of the volumes of these catches shows that the average was 3.33 c.c., but the individual catches ranged from 1.5 c.c. to 6.5 c.c., and the divergence from the average may be as great as +3.2 c.c.; and, after deducting 20 per cent. of the divergence as due to errors of the experiment,



Schutt estimates the mean variation of the plankton at about 16 per cent. above or below. This does not seem to me to indicate the uniformity that might be expected in this 'halistatic' area occupying the centre of the North Atlantic Gulf Stream circulation. Hensen also made almost simultaneous hauls with the same net in quick succession to test the amount of variation, and found that the average error was about 13 per cent.

As so much depends in all work at sea upon the weather, the conditions under which the ship is working, and the care taken in the experiment, with the view of getting further evidence under known conditions I carried out some similar experiments at Port Erin on four occasions during last April and on a further occasion a month later, choosing favourable weather and conditions of tide and wind, so as to be able to maintain an approximate position. On each of four days in April the Nansen net, with No. 20 silk, was hauled six times from the same depth (on two occasions 8 fathoms and on two occasions 20 fathoms), the hauls being taken in rapid succession and the catches being emptied from the net into bottles of 5 per cent. formaline, in which they remained until examined microscopically.

The results were of interest, for although they showed considerable uniformity in the amount of the catch—for example, six successive hauls from 8 fathoms being all of them 0.2 c.c. and four out of five from 20 fathoms being 0.6 c.c.—the volume was made up rather differently in the successive hauls. The same organisms are present for the most part in each haul, and the chief groups of organisms are present in much the same proportion. For example, in a series where the Copepoda average about 100 the Dinoflagellates average about 300 and the Diatoms about 8000, but the percentage deviation of individual hauls from the average may be as much as *plus* or *minus* 50. The numbers for each organism (about 40) in each of the twenty-six hauls have been worked out, and the details will be published elsewhere, but the conclusion I come to is that if on each occasion one haul only, in place of six, had been taken, and if one had used that haul to estimate the abundance of any one organism in that sea-area, one might have been about 50 per cent. wrong in either direction.

Successive improvements and additions to Hensen's methods in collecting plankton have been made by Lohmann, Apstein, Gran, and others, such as pumping up water of different layers through a hose-pipe and filtering it through felt, filter-paper, and other materials which retain much of the micro-plankton that escapes through the meshes of the finest silk. Use has even been made of the extraordinarily minute and beautifully regular natural filter spun by the pelagic animal *Appendicularia* for the capture of its own food. This grid-like trap,

when dissected out and examined under the microscope, reveals a surprising assemblage of the smallest protozoa and protophyta, less than 30 micro-millimetres in diameter, which would all pass easily through the meshes of our finest silk nets.

The latest refinement in capturing the minutest-known organisms of the plankton (excepting the bacteria) is a culture method devised by Dr. E. J. Allen, Director of the Plymouth Laboratory.<sup>11</sup> By diluting half a cubic centimetre of the sea-water with a considerable amount (1500 c.c.) of sterilised water treated with a nutrient solution, and distributing that over a large number (70) of small flasks in which after an interval of some days the number of different kinds of organisms which had developed in each flask were counted, he calculates that the sea contains 464,000 of such organisms per litre; and he gives reasons why his cultivations must be regarded as minimum results, and states that the total per litre may well be something like a million. Thus every new method devised seems to multiply many times the probable total population of the sea. As further results of the quantitative method it may be recorded that Brandt found about 200 diatoms per drop of water in Kiel Bay, and Hensen estimated that there are several hundred millions of diatoms under each square metre of the North Sea or the Baltic. It has been calculated that there is approximately one Copepod in each cubic inch of Baltic water, and that the annual consumption of these Copepoda by herring is about a thousand billion; and that in the 16 square miles of a certain Baltic fishery there is Copepod food for over 530 millions of herring of an average weight of 60 grammes.

There are many other problems of the plankton in addition to quantitative estimates—probably some that we have not yet recognised—and various interesting conclusions may be drawn from recent planktonic observations. Here is a case of the introduction and rapid spread of a form new to British seas.

*Biddulphia sinensis* is an exotic diatom which, according to Ostenfeld, made its appearance at the mouth of the Elbe in 1903, and spread during successive years in several directions. It appeared suddenly in our plankton gatherings at Port Erin in November 1909, and has been present in abundance each year since. Ostenfeld, in 1908, when tracing its spread in the North Sea, found that the migration to the north along the coast of Denmark to Norway corresponded with the rate of flow of the Jutland current to the Skager Rak—viz., about 17 cm. per second—a case of plankton distribution throwing light on hydrography—and he predicted that it would soon be found in the English

<sup>11</sup> *Journ. Mar. Biol. Assoc.* xii. 1, July 1919.

Channel. Dr. Marie Lebour, who recently examined the store of plankton gatherings at the Plymouth Laboratory, finds that as a matter of fact this form did appear in abundance in the collections of October 1909, within a month of the time when according to our records it reached Port Erin. Whether or not this is an Indo-Pacific species brought accidentally by a ship from the Far East, or whether it is possibly a new mutation which appeared suddenly in our seas, there is no doubt that it was not present in our Irish Sea plankton gatherings previous to 1909, but has been abundant since that year, and has completely adopted the habits of its English relations—appearing with *B. mobilensis* in late autumn, persisting during the winter, reaching a maximum in spring, and dying out before summer.

The Nauplius and Cypris stages of *Balanus* in the plankton form an interesting study. The adult barnacles are present in enormous abundance on the rocks round the coast, and they reproduce in winter, at the beginning of the year. The newly emitted young are sometimes so abundant as to make the water in the shore pools and in the sea close to shore appear muddy. The Nauplii first appeared at Port Erin, in 1907, in the bay gatherings on February 22 (in 1908 on February 13), and increased with ups and downs to their maximum on April 15, and then decreased until their disappearance on April 26. None were taken at any other time of the year. The Cypris stage follows on after the Nauplius. It was first taken in the bay on April 6, rose to its maximum on the same day with the Nauplius, and was last caught on May 24. Throughout, the Cypris curve keeps below that of the Nauplius, the maxima being 1740 and 10,500 respectively. Probably the difference between the two curves represents the death-rate of *Balanus* during the Nauplius stage. That conclusion I think we are justified in drawing, but I would not venture to use the result of any haul, or the average of a number of hauls, to multiply by the number of square yards in a zone round our coast in order to obtain an estimate of the number of young barnacles, or of the old barnacles that produced them—the irregularities are too great.

To my mind it seems clear that there must be three factors making for irregularity in the distribution of a plankton organism:—

1. The sequence of stages in its life-history—such as the Nauplius and Cypris stages of *Balanus*.
2. The results of interaction with other organisms—as when a swarm of *Calanus* is pursued and devoured by a shoal of herring.
3. Abnormalities in time or abundance due to the physical environment—as in favourable or unfavourable seasons.

And these factors must be at work in the open ocean as well as in coastal waters.

In many oceanographical inquiries there is a double object. There is the scientific interest and there is the practical utility—the interest, for example, of tracing a particular swarm of a Copepod like *Calanus*, and of making out why it is where it is at a particular time, tracing it back to its place of origin, finding that it has come with a particular body of water, and perhaps that it is feeding upon a particular assemblage of Diatoms; endeavouring to give a scientific explanation of every stage in its progress. Then there is the utility—the demonstration that the migration of the *Calanus* has determined the presence of a shoal of herrings or mackerel that are feeding upon it, and so have been brought within the range of the fisherman and have constituted a commercial fishery.

We have evidence that pelagic fish which congregate in shoals, such as herring and mackerel, feed upon the Crustacea of the plankton and especially upon Copepoda. A few years ago when the summer herring fishery off the south end of the Isle of Man was unusually near the land, the fishermen found large red patches in the sea where the fish were specially abundant. Some of the red stuff, brought ashore by the men, was examined at the Port Erin Laboratory and found to be swarms of the Copepod *Temora longicornis*; and the stomachs of the herring caught at the same time were engorged with the same organism. It is not possible to doubt that during these weeks of the herring fishery in the Irish Sea the fish were feeding mainly upon this species of Copepod. Some ten years ago Dr. E. J. Allen and Mr. G. E. Bullen published<sup>12</sup> some interesting work, from the Plymouth Marine Laboratory, demonstrating the connection between mackerel and Copepoda and sunshine in the English Channel; and Farran<sup>13</sup> states that in the spring fishery on the West of Ireland the food of the mackerel is mainly composed of *Calanus*.

Then again at the height of the summer mackerel fishery in the Hebrides, in 1913, we found<sup>14</sup> the fish feeding upon the large Copepod *Calanus finmarchicus*, which was caught in the tow-net at the rate of about 6000 in a five-minutes' haul, and 6000 was also the average number found in the stomachs of the fish caught at the same time.

These were cases where the fish were feeding upon the organism that was present in swarms—a monotonic plankton—but in other cases the fish are clearly selective in their diet. If the sardine of the French coast can pick out from the micro-plankton the minute Peridinales in preference to the equally minute Diatoms which are present in the sea at the same time, there seems no reason why the herring and the

<sup>12</sup> *Journ. Mar. Biol. Assoc.* vol. viii. (1909), pp. 394-406.

<sup>13</sup> *Conseil Internat. Bull. Trimestr.* 1902-8, 'Planktonique,' p. 89.

<sup>14</sup> 'Spolia Runiana,' iii. *Linn. Soc. Journ., Zoology*, vol. xxxiv p. 95, 1918.

mackerel should not be able to select particular species of Copepoda or other large organisms from the macro-plankton, and we have evidence that they do. Nearly thirty years ago the late Mr. Isaac Thompson, a constant supporter of the Zoological Section of this Association and one of the Honorary Local Secretaries for the last Liverpool meeting, showed me in 1893 that young plaice at Port Erin were selecting one particular Copepod, a species of *Jonesiella*, out of many others caught in our tow-nets at the time. H. Blegvad<sup>15</sup> showed in 1916 that young food fishes and also small shore fishes pick out certain species of Copepoda (such as Harpacticoids) and catch them individually—either lying in wait or searching for them. A couple of years later<sup>16</sup> Dr. Marie Lebour published a detailed account of her work at Plymouth on the food of young fishes, proving that certain fish undoubtedly do prefer certain planktonic food.

These Crustacea of the plankton feed upon smaller and simpler organisms—the Diatoms, the Peridinians, and the Flagellates—and the fish themselves in their youngest post-larval stages are nourished by the same minute forms of the plankton. Thus it appears that our sea-fisheries ultimately depend upon the living plankton which no doubt in its turn is affected by hydrographic conditions. A correlation seems to be established between the Cornish pilchard fisheries and periodic variations in the physical characters (probably the salinity) of the water of the English Channel between Plymouth and Jersey.<sup>17</sup> Apparently a diminished intensity in the Atlantic current corresponds with a diminished fishery in the following summer. Possibly the connection in these cases is through an organism of the plankton.

It is only a comparatively small number of different kinds of organisms—both plants and animals—that make up the bulk of the plankton that is of real importance to fish. One can select about half-a-dozen species of Copepoda which constitute the greater part of the summer zoo-plankton suitable as food for larval or adult fishes, and about the same number of generic types of Diatoms which similarly make up the bulk of the available spring phyto-plankton year after year. This fact gives great economic importance to the attempt to determine with as much precision as possible the times and conditions of occurrence of these dominant factors of the plankton in an average year. An obvious further extension of this investigation is an inquiry into the degree of coincidence between the times of appearance in the sea of the plankton organisms and of the young fish, and the possible effect of any marked absence of correlation in time and quantity.

Just before the war the International Council for the Exploration

<sup>15</sup> *Rep. Danish Biol. Stat.* xxiv. 1916.

<sup>16</sup> *Journ. Mar. Biol. Assoc.* May 1918.

<sup>17</sup> See E. C. Jee, *Hydrography of the English Channel*, 1904-17.

of the Sea<sup>18</sup> arrived at the conclusion that fishery investigations indicated the probability that the great periodic fluctuations in the fisheries are connected with the fish larvæ being developed in great quantities only in certain years. Consequently they advised that plankton work should be directed primarily to the question whether these fluctuations depend upon differences in the plankton production in different years. It was then proposed to begin systematic investigation of the fish larvæ and the plankton in spring and to determine more definitely the food of the larval fish at various stages.

About the same time Dr. Hjort<sup>19</sup> made the interesting suggestion that possibly the great fluctuations in the number of young fish observed from year to year may not depend wholly upon the number of eggs produced, but also upon the relation in time between the hatching of these eggs and the appearance in the water of the enormous quantity of Diatoms and other plant plankton upon which the larval fish after the absorption of their yolk depend for food. He points out that, if even a brief interval occurs between the time when the larvæ first require extraneous nourishment and the period when such food is available, it is highly probable that an enormous mortality would result. In that case even a rich spawning season might yield but a poor result in fish in the commercial fisheries of successive years for some time to come. So that, in fact, the numbers of a year-class may depend not so much upon a favourable spawning season as upon a coincidence between the hatching of the larvæ and the presence of abundance of phyto-plankton available as food<sup>20</sup>

The curve for the spring maximum of Diatoms corresponds in a general way with the curve representing the occurrence of pelagic fish eggs in our seas. But is the correspondence sufficiently exact and constant to meet the needs of the case? The phyto-plankton may still be relatively small in amount during February and part of March in some years, and it is not easy to determine exactly when, in the open sea, the fish eggs have hatched out in quantity and the larvæ have absorbed their food-yolk and started feeding on Diatoms.

If, however, we take the case of one important fish—the plaice—we can get some data from our hatching experiments at the Port Erin Biological Station which have now been carried on for a period of seventeen years. An examination of the hatchery records for these years in comparison with the plankton records of the neighbouring sea, which have been kept systematically for the fourteen years from 1907

<sup>18</sup> *Rapports et Proc. Verb.* xix. December 1913.

<sup>19</sup> *Rapports et Proc. Verb.* xx. 1914, p. 204.

<sup>20</sup> For the purpose of this argument we may include in 'phyto-plankton' the various groups of Flagellata and other minute organisms which may be present with the Diatoms

to 1920 inclusive, shows that in most of these years the Diatoms were present in abundance in the sea a few days at least before the fish larvæ from the hatchery were set free, and that it was only in four years (1908, '09, '13, and '14) that there was apparently some risk of the larvæ finding no phyto-plankton food, or very little. The evidence so far seems to show that if fish larvæ are set free in the sea as late as March 20, they are fairly sure of finding suitable food;<sup>21</sup> but if they are hatched as early as February they run some chance of being starved.

But this does not exhaust the risks to the future fishery. C. G. Joh. Petersen and Boysen-Jensen in their valuation of the Limfjord<sup>22</sup> have shown that in the case not only of some fish but also of the larger invertebrates on which they feed there are marked fluctuations in the number of young produced in different seasons, and that it is only at intervals of years that a really large stock of young is added to the population.

The prospects of a year's fishery may therefore depend primarily upon the rate of spawning of the fish, affected no doubt by hydrographic and other environmental conditions, secondarily upon the presence of a sufficient supply of phyto-plankton in the surface layers of the sea at the time when the fish larvæ are hatched, and that in its turn depends upon photosynthesis and physico-chemical changes in the water, and finally upon the reproduction of the stock of molluscs or worms at the bottom which constitute the fish food at later stages of growth and development.

The question has been raised of recent years—Is there enough plankton in the sea to provide sufficient nourishment for the larger animals, and especially for those fixed forms such as sponges that are supposed to feed by drawing currents of plankton-laden water through the body? In a series of remarkable papers from 1907 onwards Pütter and his followers put forward the views (1) that the carbon requirements of such animals could not be met by the amount of plankton in the volume of water that could be passed through the body in a given time, and (2) that sea-water contained a large amount of dissolved organic carbon compounds which constitute the chief if not the only food of a large number of marine animals. These views have given rise to much controversy and have been useful in stimulating further research, but I believe it is now admitted that Pütter's samples of water from the Bay of Naples and at Kiel were probably polluted, that his figures were erroneous, and that his conclusions

<sup>21</sup> All dates and statements as to occurrence refer to the Irish Sea round the south end of the Isle of Man. For further details see *Report Lancs. Sea-Fish. Lab.* for 1919.

<sup>22</sup> *Report of Danish Biol. Station for 1919.*

must be rejected, or at least greatly modified. His estimates of the plankton were minimum ones, while it seems probable that his figures for the organic carbon present represent a variable amount of organic matter arising from one of the reagents used in the analyses.<sup>23</sup> The later experimental work of Henze, of Raben, and of Moore shows that the organic carbon dissolved in sea-water is an exceedingly minute quantity, well within the limits of experimental error. Moore puts it, at the most, at one-millionth part, or 1 mgm. in a litre. At the Dundee meeting of the Association in 1912 a discussion on this subject took place, at which Pütter still adhered to a modified form of his hypothesis of the inadequacy of the plankton and the nutrition of lower marine animals by the direct absorption of dissolved organic matter. Further work at Port Erin since has shown that, while the plankton supply as found generally distributed would prove sufficient for the nutrition of such sedentary animals as Sponges and Ascidians, which require to filter only about fifteen times their own volume of water per hour, it is quite inadequate for active animals such as Crustaceans and Fishes. These latter are, however, able to seek out and capture their food, and are not dependent on what they may filter or absorb from the sea-water. This result accords well with recorded observations on the irregularity in the distribution of the plankton, and with the variations in the occurrence of the migratory fishes which may be regarded as following and feeding upon the swarms of planktonic organisms.

This then, like most of the subjects I am dealing with, is still a matter of controversy, still not completely understood. Our need, then, is Research, more Research, and *still more Research*.

Our knowledge of the relations between plankton productivity and variation and the physico-chemical environment is still in its infancy, but gives promise of great results in the hands of the bio-chemist and the physical chemist.

Recent papers by Sørensen, Palitzsch, Witting, Moore, and others have made clear that the amount of hydrogen-ion concentration as indicated by the relative degree of alkalinity and acidity in the sea-water may undergo local and periodic variations and that these have an effect upon the living organisms in the water and can be correlated with their presence and abundance. To take an example from our own seas, Professor Benjamin Moore and his assistants in their work at the Port Erin Biological Station in successive years from 1912 onwards have shown<sup>24</sup> that the sea around the Isle of Man is a good deal more alkaline in spring (say April) than it is in summer (say

<sup>23</sup> See Moore, etc., *Bio-Chem Journ.* vi. p. 266, 1912.

<sup>24</sup> 'Photosynthetic phenomena in sea-water.' *Trans. Liverpool Biol. Soc.* xxix. 233, 1915.



July). The alkalinity, which gets low in summer, increases somewhat in autumn, and then decreases rapidly, to disappear during the winter; and then once more, after several months of a minimum, begins to come into evidence again in March, and rapidly rises to its maximum in April or May. This periodic change in alkalinity will be seen to correspond roughly with the changes in the living microscopic contents of the sea represented by the phyto-plankton annual curve, and the connection between the two will be seen when we realise that the alkalinity of the sea is due to the relative absence of carbon dioxide. In early spring, then, the developing myriads of diatoms in their metabolic processes gradually use up the store of carbon dioxide accumulated during the winter, or derived from the bi-carbonates of calcium and magnesium, and so increase the alkalinity of the water, till the maximum of alkalinity, due to the fixation of the carbon and the reduction in amount of carbon dioxide, corresponds with the crest of the phyto-plankton curve in, say, April. Moore has calculated that the annual turnover in the form of carbon which is used up or converted from the inorganic into an organic form probably amounts to something of the order of 20,000 or 30,000 tons of carbon per cubic mile of sea-water, or, say, over an area of the Irish Sea measuring 16 square miles and a depth of 50 fathoms; and this probably means a production each season of about two tons of dry organic matter, corresponding to at least ten tons of moist vegetation, per acre—which suggests that we may still be very far from getting from our seas anything like the amount of possible food-matters that are produced annually.

Testing the alkalinity of the sea-water may therefore be said to be merely ascertaining and measuring the results of the photosynthetic activity of the great phyto-plankton rise in spring due to the daily increase of sunlight.

The marine biologists of the Carnegie Institute, Washington, have made a recent contribution to the subject in certain observations on the alkalinity of the sea (as determined by hydrogen-ion concentration), during which they found in tropical mid-Pacific a sudden change to acidity in a current running eastwards. Now in the Atlantic the Gulf Stream, and tropical Atlantic waters generally, are much more alkaline than the colder coastal water running south from the Gulf of St. Lawrence. That is, the colder Arctic water has more carbon dioxide. This suggests that the Pacific easterly set may be due to deeper water, containing more carbon dioxide (=acidity), coming to the surface at that point. The alkalinity of the sea-water can be determined rapidly by mixing the sample with a few drops of an indicator and observing the change of colour; and this method of detecting ocean currents by observing the hydrogen-ion concentration of the water might be useful to navigators as showing the time of entrance to a known current.

Oceanography has many practical applications—chiefly, but by no means wholly, on the biological side. The great fishing industries of the world deal with living organisms, of which all the vital activities and the inter-relations with the environment are matters of scientific investigation. Aquiculture is as susceptible of scientific treatment as agriculture can be; and the fisherman who has been in the past too much the nomad and the hunter—if not, indeed, the devastating raider—must become in the future the settled farmer of the sea if his harvest is to be less precarious. Perhaps the nearest approach to cultivation of a marine product, and of the fisherman reaping what he has actually sown, is seen in the case of the oyster and mussel industries on the west coast of France, in Holland, America, and to a less extent on our own coast. Much has been done by scientific men for these and other similar coastal fisheries since the days when Professor Coste in France in 1859 introduced oysters from the Scottish oyster-beds to start the great industry at Arcachon and elsewhere. Now we buy back the descendants of our own oysters from the French ostreiculturists to replenish our depleted beds.

It is no small matter to have introduced a new and important food-fish to the markets of the world. The remarkable deep-water “tile-fish,” new to science and described as *Lopholatilus chamaeleonticeps*, was discovered in 1879 by one of the United States fishing schooners to the south of Nantucket, near the 100-fathom line. Several thousand pounds weight were caught, and the matter was duly investigated by the United States Fish Commission. For a couple of years after that the fish was brought to market in quantity, and then something unusual happened at the bottom of the sea, and in 1882 millions of dead tile-fish were found floating on the surface over an area of thousands of square miles. The schooner *Navarino* sailed for two days and a night through at least 150 miles of sea, thickly covered as far as the eye could reach with dead fish, estimated at 256,000 to the square mile. The Fish Commission sent a vessel to fish systematically over the grounds known as the ‘Gulf Stream slope,’ where the tile-fish had been so abundant during the two previous years, but she did not catch a single fish, and the associated sub-tropical invertebrate fauna was also practically obliterated.

This wholesale destruction was attributed by the American oceanographers to a sudden change in the temperature of the water at the bottom, due in all probability to a withdrawal southwards of the warm Gulf Stream water and a flooding of the area by the cold Labrador current.

I am indebted to Dr. C. H. Townsend, Director of the celebrated New York Aquarium, for the latest information in regard to the

reappearance in quantity of this valuable fish upon the old fishing grounds off Nantucket and Long Island, at about 100 miles from the coast to the east and south-east of New York. It is believed that the tile-fish is now abundant enough to maintain an important fishery, which will add an excellent food-fish to the markets of the United States. It is easily caught with lines at all seasons of the year, and reaches a length of over three feet and a weight of 40 to 50 pounds. During July 1915 the product of the fishery was about two and a half million pounds weight, valued at 55,000 dollars, and in the first few months of 1917 the catch was four and a half million pounds, for which the fishermen received 247,000 dollars.

We can scarcely hope in European seas to add new food-fishes to our markets, but much may be done through the co-operation of scientific investigators of the ocean with the Administrative Departments to bring about a more rational conservation and exploitation of the national fisheries.

Earlier in this address I referred to the pioneer work of the distinguished Manx naturalist, Professor Edward Forbes. There are many of his writings and of his lectures which I have no space to refer to which have points of oceanographic interest. Take this, for example, in reference to our national sea fisheries. We find him in 1847 writing to a friend: 'On Friday night I lectured at the Royal Institution. The subject was the bearing of submarine researches and distribution matters on the fishery question. I pitched into Government mismanagement pretty strong, and made a fair case of it. It seems to me that at a time when half the country is starving we are utterly neglecting or grossly mismanaging great sources of wealth and food. . . . Were I a rich man I would make the subject a hobby, for the good of the country and for the better proving that the true interests of Government are those linked with and inseparable from Science.' We must still cordially approve of these last words, while recognising that our Government Department of Fisheries is now being organised on better lines, is itself carrying on scientific work of national importance, and is, I am happy to think, in complete sympathy with the work of independent scientific investigators of the sea and desirous of closer co-operation with University laboratories and biological stations.

During recent years one of the most important and most frequently discussed of applications of fisheries investigation has been the productivity of the trawling grounds, and especially those of the North Sea. It has been generally agreed that the enormous increase of fishing power during the last forty years or so has reduced the number of large plaice, so that the average size of that fish caught in our home

waters has become smaller, although the total number of plaice landed had continued to increase up to the year of the outbreak of war. Since then, from 1914 to 1919, there has of necessity been what may be described as the most gigantic experiment ever seen in the closing of extensive fishing grounds. It is still too early to say with any certainty exactly what the results of that experiment have been, although some indications of an increase of the fish population in certain areas have been recorded. For example, the Danes, A. C. Johansen and Kirstine Smith, find that large plaice landed in Denmark are now more abundant, and they attribute this to a reversal of the pre-war tendency, due to less intensive fishing. But Dr. James Johnstone has pointed out that there is some evidence of a natural periodicity in abundance of such fish and that the results noticed may represent phases in a cyclic change. If the periodicity noted in Liverpool Bay<sup>25</sup> holds good for other grounds it will be necessary in any comparison of pre-war and post-war statistics to take this natural variation in abundance into very careful consideration.

In the application of oceanographic investigations to sea-fisheries problems, one ultimate aim, whether frankly admitted or not, must be to obtain some kind of a rough approximation to a census or valuation of the sea—of the fishes that form the food of man, of the lower animals of the sea-bottom on which many of the fishes feed, and of the planktonic contents of the upper waters which form the ultimate organised food of the sea—and many attempts have been made in different ways to attain the desired end.

Our knowledge of the number of animals living in different regions of the sea is for the most part relative only. We know that one haul of the dredge is larger than another, or that one locality seems richer than another, but we have very little information as to the actual numbers of any kind of animal per square foot or per acre in the sea. Hensen, as we have seen, attempted to estimate the number of food-fishes in the North Sea from the number of their eggs caught in a comparatively small series of hauls of the tow-net, but the data were probably quite insufficient and the conclusions may be erroneous. It is an interesting speculation to which we cannot attach any economic importance. Heincke says of it: 'This method appears theoretically feasible, but presents in practice so many serious difficulties that no positive results of real value have as yet been obtained.'

All biologists must agree that to determine even approximately the number of individuals of any particular species living in a known area is a contribution to knowledge which may be of great economic value

<sup>25</sup> See Johnstone, *Report Lancs. Sea-Fish. Lab.* for 1917, p. 60; and Daniel, *Report* for 1919, p. 51.

in the case of the edible fishes, but it may be doubted whether Hensen's methods, even with greatly increased data, will ever give us the required information. Petersen's method, of setting free marked plaice and then assuming that the proportion of these recaught is to the total number marked as the fishermen's catch in the same district is to the total population, will only hold good in circumscribed areas where there is practically no migration and where the fish are fairly evenly distributed. This method gives us what has been called 'the fishing coefficient,' and this has been estimated for the North Sea to have a probable value of about 0.33 for those sizes of fish which are caught by the trawl. Heincke,<sup>26</sup> from an actual examination of samples of the stock on the ground obtained by experimental trawling ('the catch coefficient'), supplemented by the market returns of the various countries, estimates the adult plaice at about 1,500 millions, of which about 500 millions are caught or destroyed by the fishermen annually.

It is difficult to imagine any further method which will enable us to estimate any such case as, say, the number of plaice in the North Sea where the individuals are so far beyond our direct observation and are liable to change their positions at any moment. But a beginning can be made on more accessible ground with more sedentary animals, and Dr. C. G. Joh. Petersen, of the Danish Biological Station, has for some years been pursuing the subject in a series of interesting Reports on the 'Evaluation of the Sea.'<sup>27</sup> He uses a bottom-sampler, or grab, which can be lowered down open and then closed on the bottom so as to bring up a sample square foot or square metre (or in deep water one-tenth of a square metre) of the sand or mud and its inhabitants. With this apparatus, modified in size and weight for different depths and bottoms, Petersen and his fellow-workers have made a very thorough examination of the Danish waters, and especially of the Kattegat and the Limfjord, have described a series of 'animal communities' characteristic of different zones and regions of shallow water, and have arrived at certain numerical results as to the quantity of animals in the Kattegat expressed in tons—such as 5,000 tons of plaice requiring as food 50,000 tons of 'useful animals' (mollusca and polychaet worms), and 25,000 tons of starfish using up 200,000 tons of useful animals which might otherwise serve as food for fishes, and the dependence of all these animals directly or indirectly upon the great Beds of *Zostera*, which make up 24,000,000 tons in Kattegat. Such estimates are obviously of great biological interest, and even if only rough approximations are a valuable contribution to our under-

<sup>26</sup> F. Heincke, *Cons. Per. Internat. Explor. de la Mer*, 'Investigations on the Plaice,' Copenhagen, 1913.

<sup>27</sup> See *Reports of the Danish Biological Station*, and especially the Report for 1918 'The Sea Bottom and its Production of Fish Food.'

standing of the metabolism of the sea and of the possibility of increasing the yield of local fisheries.

But on studying these Danish results in the light of what we know of our own marine fauna, although none of our seas have been examined in the same detail by the bottom-sampler method, it seems probable that the animal communities as defined by Petersen are not exactly applicable on our coasts and that the estimates of relative and absolute abundance may be very different in different seas under different conditions. The work will have to be done in each great area, such as the North Sea, the English Channel, and the Irish Sea, independently. This is a necessary investigation, both biological and physical, which lies before the oceanographers of the future, upon the results of which the future preservation and further cultivation of our national sea-fisheries may depend.

It has been shown by Johnstone and others that the common edible animals of the shore may exist in such abundance that an area of the sea may be more productive of food for man than a similar area of pasture or crops on land. A Lancashire mussel bed has been shown to have as many as 16,000 young mussels per square foot, and it is estimated that in the shallow waters of Liverpool Bay there are from twenty to 200 animals of sizes varying from an amphipod to a plaice on each square metre of the bottom.<sup>28</sup>

From these and similar data which can be readily obtained, it is not difficult to calculate totals by estimating the number of square yards in areas of similar character between tide-marks or in shallow water. And from weighings of samples some approximation to the number of tons of available food may be computed. But one must not go too far. Let all the figures be based upon actual observation. Imagination is necessary in science, but in calculating a population of even a very limited area it is best to believe only what one can see and measure.

Countings and weighings, however, do not give us all the information we need. It is something to know even approximately the number of millions of animals on a mile of shore and the number of millions of tons of possible food in a sea-area, but that is not sufficient. All food-fishes are not equally nourishing to man, and all plankton and bottom invertebrata are not equally nourishing to a fish. At this point the biologist requires the assistance of the physiologist and the bio-chemist. We want to know next the value of our food matters in proteids, carbohydrates, and fats, and the resulting calories. Dr. Johnstone, of the Oceanography Department of the University of Liverpool, has already shown us how markedly a fat summer herring

<sup>28</sup> *Conditions of Life in the Sea*, Cambridge Univ. Press, 1908.

differs in essential constitution from the ordinary white fish, such as the cod, which is almost destitute of fat.

Professor Brandt, at Kiel, Professor Benjamin Moore, at Port Erin, and others have similarly shown that plankton gatherings may vary greatly in their nutrient value according as they are composed mainly of Diatoms, of Dinoflagellates, or of Copepoda. And, no doubt, the animals of the 'benthos,' the common invertebrates of our shores, will show similar differences in analysis.<sup>29</sup> It is obvious that some contain more solid flesh, others more water in their tissues, others more calcareous matter in the exoskeleton, and that therefore weight for weight we may be sure that some are more nutritious than the others; and this is probably at least one cause of that preference we see in some of our bottom-feeding fish for certain kinds of food, such as polychaet worms, in which there is relatively little waste, and thin-shelled lamellibranch molluscs, such as young mussels, which have a highly nutrient body in a comparatively thin and brittle shell.

My object in referring to these still incomplete investigations is to direct attention to what seems a natural and useful extension of faunistic work, for the purpose of obtaining some approximation to a quantitative estimate of the more important animals of our shores and shallow water and their relative values as either the immediate or the ultimate food of marketable fishes.

Each such fish has its 'food-chain' or series of alternative chains, leading back from the food of man to the invertebrates upon which it preys and then to the food of these, and so down to the smallest and simplest organisms in the sea, and each such chain must have all its links fully worked out as to seasonal and quantitative occurrence back to the Diatoms and Flagellates which depend upon physical conditions and take us beyond the range of biology—but not beyond that of oceanography. The Diatoms and the Flagellates are probably more important than the more obvious sea-weeds not only as food, but also in supplying to the water the oxygen necessary for the respiration of living protoplasm. Our object must be to estimate the rate of production and rate of destruction of all organic substances in the sea.

To attain to an approximate census and valuation of the sea—remote though it may seem—is a great aim, but it is not sufficient. We want not only to observe and to count natural objects, but also to understand them. We require to know not merely what an organism is—in the fullest detail of structure and development and affinities—

<sup>29</sup> Moore and others have made analyses of the protein, fat, etc., in the soft parts of Sponge, Ascidian, Aplysia, Fusus, Echinus and Cancer at Port Erin, and find considerable differences—the protein ranging, for example, from 8 to 51 per cent., and the fat from 2 to 14 per cent. (see *Bio-Chemical Journ.* vi. p. 291).

and also where it occurs—again in full detail—and in what abundance under different circumstances, but also *how* it lives and what all its relations are to both its physical and its biological environment, and that is where the physiologist, and especially the bio-chemist, can help us. In the best interests of biological progress the day of the naturalist who merely collects, the day of the anatomist and histologist who merely describe, is over, and the future is with the observer and the experimenter animated by a divine curiosity to enter into the life of the organism and understand how it lives and moves and has its being. 'Happy indeed is he who has been able to discover the causes of things.'

Cardiff is a sea-port, and a great sea-port, and the Bristol Channel is a notable sea-fisheries centre of growing importance. The explorers and merchant venturers of the South-West of England are celebrated in history. What are you doing now in Cardiff to advance our knowledge of the ocean? You have here an important university centre and a great modern national museum, and either or both of these homes of research might do well to establish an oceanographical department, which would be an added glory to your city and of practical utility to the country. This is the obvious centre in Wales for a sea-fisheries institute for both research and education. Many important local movements have arisen from British Association meetings, and if such a notable scientific development were to result from the Cardiff meeting of 1920, all who value the advance of knowledge and the application of knowledge to industry would applaud your enlightened action.

But in a wider sense, it is not to the people of Cardiff alone that I appeal, but to the whole population of these Islands, a maritime people who owe everything to the sea. I urge them to become better informed in regard to our national sea-fisheries and take a more enlightened interest in the basal principles that underlie a rational regulation and exploitation of these important industries. National efficiency depends to a very great extent upon the degree in which scientific results and methods are appreciated by the people and scientific investigation is promoted by the Government and other administrative authorities. The principles and discoveries of science apply to aquiculture no less than to agriculture. To increase the harvest of the sea the fisheries must be continuously investigated, and such cultivation as is possible must be applied, and all this is clearly a natural application of the biological and hydrographical work now united under the science of Oceanography.





# British Association for the Advancement of Science.

SECTION A : CARDIFF, 1920.

## ADDRESS

TO THE

## MATHEMATICAL AND PHYSICAL SCIENCE SECTION

BY

PROFESSOR A. S. EDDINGTON, M.A., M.Sc., F.R.S.,  
PRESIDENT OF THE SECTION.

### *The Internal Constitution of the Stars.*

LAST year at Bournemouth we listened to a proposal from the President of the Association to bore a hole in the crust of the earth and discover the conditions deep down below the surface. This proposal may remind us that the most secret places of Nature are, perhaps, not 10 to the  $n$ -th miles above our heads, but 10 miles below our feet. In the last five years the outward march of astronomical discovery has been rapid, and the most remote worlds are now scarcely safe from its inquisition. By the work of H. Shapley the globular clusters, which are found to be at distances scarcely dreamt of hitherto, have been explored, and our knowledge of them is in some respects more complete than that of the local aggregation of stars which includes the Sun. Distance lends not enchantment but precision to the view. Moreover, theoretical researches of Einstein and Weyl make it probable that the space which remains beyond is not illimitable; not merely the material universe, but space itself, is perhaps finite; and the explorer must one day stay his conquering march for lack of fresh realms to invade. But to-day let us turn our thoughts inwards to that other region of mystery—a region cut off by more substantial barriers, for, contrary to many anticipations, even the discovery of the fourth dimension has not enabled us to get at the inside of a body. Science has material and non-material appliances to bore into the interior, and I have chosen to devote this address to what may be described as analytical boring devices—*absit omen*!

The analytical appliance is delicate at present, and, I fear, would make little headway against the solid crust of the earth. Instead of letting it blunt itself against the rocks, let us look round for something easier to penetrate. The Sun? Well, perhaps. Many have struggled to penetrate the mystery of the interior of the Sun; but the difficulties are great, for its substance is denser than water. It may not be quite so bad as Biron makes out in *Love's Labour's Lost*:—

The heaven's glorious sun,  
That will not be deep-searched with saucy looks;  
Small have continual plodders ever won  
Save base authority from others' books.

But it is far better if we can deal with matter in that state known as a perfect gas, which charms away difficulties as by magic. Where shall it be found?

A few years ago we should have been puzzled to say where, except perhaps in certain nebulae; but now it is known that abundant material of this kind awaits investigation. Stars in a truly gaseous state exist in great numbers, although at first sight they are scarcely to be discriminated from dense stars like our Sun. Not only so, but the gaseous stars are the most powerful light-givers, so that they force themselves on our attention. Many of the familiar stars are of this kind—Aldebaran, Canopus, Arcturus, Antares; and it would be safe to say that three-quarters of the naked-eye stars are in this diffuse state. This remarkable condition has been made known through the researches of H. N. Russell<sup>1</sup> and E. Hertzsprung; the way in which their conclusions, which ran counter to the prevailing thought of the time, have been substantiated on all sides by overwhelming evidence, is the outstanding feature of recent progress in stellar astronomy.

The diffuse gaseous stars are called *giants*, and the dense stars are called *dwarfs*. During the life of a star there is presumably a gradual increase of density through contraction, so that these terms distinguish the earlier and later stages of stellar history. It appears that a star begins its effective life as a giant of comparatively low temperature—a red or M-type star. As this diffuse mass of gas contracts its temperature must rise, a conclusion long ago pointed out by Homer Lane. The rise continues until the star becomes too dense, and ceases to behave as a perfect gas. A maximum temperature is attained, depending on the mass, after which the star, which has now become a dwarf, cools and further contracts. Thus each temperature-level is passed through twice, once in an ascending and once in a descending stage—once as a giant, once as a dwarf. Temperature plays so predominant a part in the usual spectral classification that the ascending and descending stars were not originally discriminated, and the customary classification led to some perplexities. The separation of the two series was discovered through their great difference in luminosity, particularly striking in the case of the red and yellow stars, where the two stages fall widely apart in the star's history. The bloated giant has a far larger surface than the compact dwarf, and gives correspondingly greater light. The distinction was also revealed by direct determinations of stellar densities, which are possible in the case of eclipsing variables like Algol. Finally, Adams and Kohlschütter have set the seal on this discussion by showing that there are actual spectral differences between the ascending and descending stars at the same temperature-level, which are conspicuous enough—when they are looked for.

Perhaps we should not too hastily assume that the direction of evolution is necessarily in the order of increasing density, in view of our ignorance of the origin of a star's heat, to which I must allude later. But, at any rate, it is a great advance to have disentangled what

<sup>1</sup> *Nature*, vol. 93, pp. 227, 252, 281.

is the true order of continuous increase of density, which was hidden by superficial resemblances.

The giant stars, representing the first half of a star's life, are taken as material for our first boring experiment. Probably, measured in time, this stage corresponds to much less than half the life, for here it is the ascent which is easy and the way down is long and slow. Let us try to picture the conditions inside a giant star. We need not dwell on the vast dimensions—a mass like that of the Sun, but swollen to much greater volume on account of the low density, often below that of our own atmosphere. It is the star as a storehouse of heat which especially engages our attention. In the hot bodies familiar to us the heat consists in the energy of motion of the ultimate particles, flying at great speeds hither and thither. So too in the stars a great store of heat exists in this form; but a new feature arises. A large proportion, sometimes more than half the total heat, consists of imprisoned radiant energy—ether-waves travelling in all directions trying to break through the material which encages them. The star is like a sieve, which can only retain them temporarily; they are turned aside, scattered, absorbed for a moment, and flung out again in a new direction. An element of energy may thread the maze for hundreds of years before it attains the freedom of outer space. Nevertheless the sieve leaks, and a steady stream permeates outwards, supplying the light and heat which the star radiates all round.

That some ethereal heat as well as material heat exists in any hot body would naturally be admitted; but the point on which we have here to lay stress is that in the stars, particularly in the giant stars, the ethereal portion rises to an importance which quite transcends our ordinary experience, so that we are confronted with a new type of problem. In a red-hot mass of iron the ethereal energy constitutes less than a billionth part of the whole; but in the tussle between matter and ether the ether gains a larger and larger proportion of the energy as the temperature rises. This change in proportion is rapid, the ethereal energy increasing rigorously as the fourth power of the temperature, and the material energy roughly as the first power. But even at the temperature of some millions of degrees attained inside the stars there would still remain a great disproportion; and it is the low density of material, and accordingly reduced material energy per unit volume in the giant stars, which wipes out the last few powers of 10. In all the giant stars known to us, widely as they differ from one another, the conditions are just reached at which these two varieties of heat-energy have attained a rough equality; at any rate one cannot be neglected compared with the other. Theoretically there could be conditions in which the disproportion was reversed and the ethereal far out-weighted the material energy; but we do not find them in the stars. It is as though the stars had been measured out—that their sizes had been determined—with a view to this balance of power; and one cannot refrain from attributing to this condition a deep significance in the evolution of the cosmos into separate stars.

To recapitulate. We are acquainted with heat in two forms—the energy of motion of material atoms and the energy of ether waves. In

familiar hot bodies the second form exists only in insignificant quantities. In the giant stars the two forms are present in more or less equal proportions. That is the new feature of the problem.

On account of this new aspect of the problem the first attempts to penetrate the interior of a star are now seen to need correction. In saying this we do not depreciate the great importance of the early researches of Lane, Ritter, Emden, and others, which not only pointed the way for us to follow, but achieved conclusions of permanent value. One of the first questions they had to consider was by what means the heat radiated into space was brought up to the surface from the low level where it was stored. They imagined a bodily transfer of the hot material to the surface by currents of convection, as in our own atmosphere. But actually the problem is, not how the heat can be brought to the surface, but how the heat in the interior can be held back sufficiently—how it can be barred in and the leakage reduced to the comparatively small radiation emitted by the stars. Smaller bodies have to manufacture the radiant heat which they emit, living from hand to mouth; the giant stars merely leak radiant heat from their store. I have put that much too crudely; but perhaps it suggests the general idea.

The recognition of ethereal energy necessitates a twofold modification in the calculations. In the first place, it abolishes the supposed convection currents; and the type of equilibrium is that known as radiative instead of convective. This change was first suggested by R. A. Sampson so long ago as 1894. The detailed theory of radiative equilibrium is particularly associated with K. Schwarzschild, who applied it to the Sun's atmosphere. It is perhaps still uncertain whether it holds strictly for the atmospheric layers, but the arguments for its validity in the interior of a star are far more cogent. Secondly, the outflowing stream of ethereal energy is powerful enough to exert a *direct mechanical effect* on the equilibrium of a star. It is as though a strong wind were rushing outwards. In fact we may fairly say that the stream of radiant energy is a wind; for though ether waves are not usually classed as material, they have the chief mechanical properties of matter, viz. mass and momentum. This wind distends the star and relieves the pressure on the inner parts. The pressure on the gas in the interior is not the full weight of the superincumbent columns, because that weight is partially borne by the force of the escaping ether waves beating their way out. This force of radiation-pressure, as it is called, makes an important difference in the formulation of the conditions for equilibrium of a star.

Having revised the theoretical investigations in accordance with these considerations,<sup>2</sup> we are in a position to deduce some definite numerical results. On the observational side we have fairly satisfactory knowledge of the masses and densities of the stars and of the total radiation emitted by them; this knowledge is partly individual and partly statistical. The theoretical analysis connects these observational data on the one hand with the physical properties of the material inside

<sup>2</sup> *Astrophysical Journal*, vol. 48, p. 205.

the star on the other hand. We can thus find certain information as to the inner material, as though we had actually bored a hole. So far as can be judged there are only two physical properties of the material which can concern us—always provided that it is sufficiently rarefied to behave as a perfect gas—viz. the average molecular weight and the transparency or permeability to radiant energy. In connecting these two unknowns with the quantities given directly by astronomical observation we depend entirely on the well-tried principles of conservation of momentum and the second law of thermodynamics. If any element of speculation remains in this method of investigation, I think it is no more than is inseparable from every kind of theoretical advance.

We have, then, on the one side the mass, density and output of heat, quantities as to which we have observational knowledge; on the other side, molecular weight and transparency, quantities which we want to discover.

To find the transparency of stellar material to the radiation traversing it is of particular interest because it links on this astronomical inquiry to physical investigations now being carried on in the laboratory, and to some extent it extends those investigations to conditions unattainable on the earth. At high temperatures the ether waves are mainly of very short wave-length, and in the stars we are dealing mainly with radiation of wave-length 3 to 30 Angström units, which might be described as very soft *x*-rays. It is interesting, therefore, to compare the results with the absorption of the harder *x*-rays dealt with by physicists. To obtain an exact measure of this absorption in the stars we have to assume a value of the molecular weight; but fortunately the extreme range possible for the molecular weight gives fairly narrow limits for the absorption. The average weight of the ultimate independent particles in a star is probably rather low, because in the conditions prevailing there the atoms would be strongly ionised; that is to say, many of the outer electrons of the system of the atom would be broken off; and as each of these free electrons counts as an independent molecule for the present purposes, this brings down the average weight. In the extreme case (probably not reached in a star) when the whole of the electrons outside the nucleus are detached the average weight comes down to about 2, *whatever the material*, because the number of electrons is about half the atomic weight for all the elements (except hydrogen). We may, then, safely take 2 as the extreme lower limit. For an upper limit we might perhaps take 200; but to avoid controversy we shall be generous and merely assume that the molecular weight is not greater than—infinity. Here is the result:—

For molecular weight 2, mass-coefficient of absorption = 10  
C.G.S. units.

For molecular weight  $\infty$ , mass-coefficient of absorption = 130  
C.G.S. units.

The true value, then, must be between 10 and 130. Partly from thermodynamical considerations, and partly from further comparisons of astronomical observation with theory, the most likely value seems to be about 35 C.G.S. units, corresponding to molecular weight 3.5.

Now this is of the same order of magnitude as the absorption of  $x$ -rays measured in the laboratory. I think the result is in itself of some interest, that in such widely different investigations we should approach the same kind of value of the opacity of matter to radiation. The penetrating power of the radiation in the star is much like that of  $x$ -rays; more than half is absorbed in a path of 20 cms. at atmospheric density. Incidentally, this very high opacity explains why a star is so nearly heat tight, and can store vast supplies of heat with comparatively little leakage.

So far this agrees with what might have been anticipated; but there is another conclusion which physicists would probably not have foreseen. The giant series comprises stars differing widely in their densities and temperatures, those at one end of the series being on the average about ten times hotter throughout than those at the other end. By the present investigation we can compare directly the opacity of the hottest stars with that of the coolest stars. The rather surprising result emerges that the opacity is the same for all; at any rate there is no difference large enough for us to detect. There seems no room for doubt that at these high temperatures the absorption-coefficient is approaching a limiting value, so that over a wide range it remains practically constant. With regard to this constancy, it is to be noted that the temperature is concerned twice over: it determines the character and wave-length of the radiation to be absorbed, as well as the physical condition of the material which is absorbing. From the experimental knowledge of  $x$ -rays we should have expected the absorption to vary very rapidly with the wave length, and therefore with the temperature. It is surprising, therefore, to find a nearly constant value.

The result becomes a little less mysterious when we consider more closely the nature of absorption. Absorption is not a continuous process, and after an atom has absorbed its quantum it is put out of action for a time until it can recover its original state. We know very little of what determines the rate of recovery of the atom, but it seems clear that there is a limit to the amount of absorption that can be performed by an atom in a given time. When that limit is reached no increase in the intensity of the incident radiation will lead to any more absorption. There is in fact a saturation effect. In the laboratory experiments the radiation used is extremely weak; the atom is practically never caught unprepared, and the absorption is proportional to the incident radiation. But in the stars the radiation is very intense and the saturation effect comes in.

Even granting that the problem of absorption in the stars involves this saturation effect, which does not affect laboratory experiments, it is not very easy to understand theoretically how the various conditions combine to give a constant absorption-coefficient independent of temperature and wave-length. But the astronomical results seem conclusive. Perhaps the most hopeful suggestion is one made to me a few years ago by C. G. Barkla. He suggested that the opacity of the stars may depend mainly on *scattering* rather than on true atomic absorption. In that case the constancy has a simple explanation, for it is known that the coefficient of scattering (unlike true absorption)

approaches a definite constant value for radiation of short wave-length. The value, moreover, is independent of the material. Further, scattering is a continuous process, and there is no likelihood of any saturation effect; thus for very intense streams of radiation its value is maintained, whilst the true absorption may sink to comparative insignificance. The difficulty in this suggestion is a numerical discrepancy between the known theoretical scattering and the values already given as deduced from the stars. The theoretical coefficient is only 0.2 compared with the observed value 10 to 130. Barkla further pointed out that the waves here concerned are not short enough to give the ideal coefficient; they would be scattered more powerfully, because under their influence the electrons in any atom would all vibrate in the same phase instead of haphazard phases. This might help to bridge the gap, but not sufficiently. It must be remembered that many of the electrons have broken loose from the atom and do not contribute to the increase.<sup>3</sup> Making all allowances for uncertainties in the data, it seems clear that the astronomical opacity is definitely higher than the theoretical scattering. Very recently, however, a new possibility has opened up which may possibly effect a reconciliation. Later in the address I shall refer to it again.

Astronomers must watch with deep interest the investigations of these short waves, which are being pursued in the laboratory, as well as the study of the conditions of ionisation both by experimental and theoretical physics, and I am glad of this opportunity of bringing before those who deal with these problems the astronomical bearing of their work.

I can only allude very briefly to the purely astronomical results which follow from this investigation;<sup>4</sup> it is here that the best opportunity occurs for checking the theory by comparison with observation, and for finding out in what respects it may be deficient. Unfortunately, the observational data are generally not very precise, and the test is not so stringent as we could wish. It turns out that (the opacity being constant) the total radiation of a giant star should be a function of its mass only, independent of its temperature or state of diffuseness. The total radiation (which is measured roughly by the luminosity) of any one star thus remains constant during the whole giant stage of its history. This agrees with the fundamental feature, pointed out by Russell in introducing the giant and dwarf hypothesis, that giant stars of every spectral type have nearly the same luminosity. From the range of luminosity of these stars it is now possible to find their range of mass. The masses are remarkably alike—a fact already suggested by work on double stars. Limits of mass in the ratio 3:1 would cover the great majority of the giant stars. Somewhat tentatively we are able to extend the investigation to dwarf stars, taking account of the

<sup>3</sup> *E.g.*, for iron non-ionised the theoretical scattering is 5.2, against an astronomical value 120. If 16 electrons (2 rings) are broken off the theoretical coefficient is 0.9 against an astronomical value 35. For different assumptions as to ionisation the values chase one another, but cannot be brought within reasonable range.

<sup>4</sup> *Monthly Notices*, vol. 77, pp. 16, 596; vol. 79, p. 2.



deviations of dense gas from the ideal laws and using our own Sun to supply a determination of the unknown constant involved. We can calculate the maximum temperature reached by different masses; for example, a star must have at least  $\frac{1}{4}$  the mass of the Sun in order to reach the lowest spectral type, M; and in order to reach the hottest type, B, it must be at least  $2\frac{1}{2}$  times as massive as the Sun. Happily for the theory no star has yet been found with a mass less than  $\frac{1}{4}$  of the Sun's; and it is a well-known fact, discovered from the study of spectroscopic binaries, that the masses of the B stars are large compared with those of other types. Again, it is possible to calculate the difference of brightness of the giant and dwarf stars of type M, *i.e.* at the beginning and end of their career; the result agrees closely with the observed difference. In the case of a class of variable stars in which the light changes seem to depend on a mechanical pulsation of the star, the knowledge we have obtained of the internal conditions enables us to predict the period of pulsation within narrow limits. For example, for  $\delta$  Cephei, the best-known star of this kind, the theoretical period is between 4 and 10 days, and the actual period is  $5\frac{1}{3}$  days. Corresponding agreement is found in all the other cases tested.

Our observational knowledge of the things here discussed is chiefly of a rather vague kind, and we can scarcely claim more than a general agreement of theory and observation. What we have been able to do in the way of tests is to offer the theory a considerable number of opportunities to 'make a fool of itself,' and so far it has not fallen into our traps. When the theory tells us that a star having the mass of the Sun will at one stage in its career reach a maximum effective temperature of  $9,000^{\circ}$  (the Sun's effective temperature being  $6,000^{\circ}$ ) we cannot do much in the way of checking it; but an erroneous theory might well have said that the maximum temperature was  $20,000^{\circ}$  (hotter than any known star), in which case we should have detected its error. If we cannot feel confident that the answers of the theory are true, it must be admitted that it has shown some discretion in lying without being found out.

It would not be surprising if individual stars occasionally depart considerably from the calculated results, because at present no serious attempt has been made to take into account rotation, which may modify the conditions when sufficiently rapid. That appears to be the next step needed for a more exact study of the question.

Probably the greatest need of stellar astronomy at the present day, in order to make sure that our theoretical deductions are starting on the right lines, is some means of measuring the apparent angular diameters of stars. At present we can calculate them approximately from theory, but there is no observational check. We believe we know with fair accuracy the apparent surface brightness corresponding to each spectral type; then all that is necessary is to divide the total apparent brightness by this surface brightness, and the result is the angular area subtended by the star. The unknown distance is not involved, because surface brightness is independent of distance. Thus the estimation of the angular diameter of any star seems to be a very simple matter. For instance, the star with the greatest apparent diameter is almost certainly

Betelgeuse, diameter '051". Next to it comes Antares, '043". Other examples are Aldebaran '022", Arcturus '020", Pollux '013". Sirius comes rather low down with diameter '007". The following table may be of interest as showing the angular diameters expected for stars of various types and visual magnitudes:—

*Probable Angular Diameters of Stars.*

Vis. Mag.	A	F	G	K	M
m.	"	"	"	"	"
0.0	·0034	·0054	·0098	·0219	·0859
2.0	·0014	·0022	·0039	·0087	·0342
4.0	·0005	·0009	·0016	·0035	·0136

However confidently we may believe in these values, it would be an immense advantage to have this first step in our deductions placed beyond doubt. If the direct measurement of these diameters could be made with any accuracy it would make a wonderfully rapid advance in our knowledge. The prospects of accomplishing some part of this task are now quite hopeful. We have learnt with great interest this year that work is being carried out by interferometer methods with the 100-inch reflector at Mount Wilson, and the results are most promising. At present the method has only been applied to measuring the separation of close double stars, but there seems to be no doubt that an angular diameter of '05" is well within reach. Although the great mirror is used for convenience, the interferometer method does not in principle require great apertures, but rather two small apertures widely separated as in a range-finder. Prof. Hale has stated, moreover, that successful results were obtained on nights of poor seeing. Perhaps it would be unsafe to assume that 'poor seeing' at Mount Wilson means quite the same thing as it does for us, and I anticipate that atmospheric disturbance will ultimately set the limit to what can be accomplished. But even if we have to send special expeditions to the top of one of the highest mountains in the world the attack on this far-reaching problem must not be allowed to languish.

I spoke earlier of the radiation-pressure exerted by the outflowing heat, which has an important effect on the equilibrium of a star. It is quite easy to calculate what proportion of the weight of the material is supported in this way; it depends neither on the density nor opacity, but solely on the star's total mass and on the molecular weight. No astronomical data are needed; the calculation involves only fundamental physical constants found by laboratory researches. Here are the figures, first for average molecular weight 3.0:—

For mass  $\frac{1}{2} \times$  Sun, fraction of weight supported by radiation-pressure = '044.

For mass  $5 \times$  Sun, fraction of weight supported by radiation-pressure = '457.

For molecular weight 5.0 the corresponding fractions are '182 and '645.

The molecular weight can scarcely go beyond this range,<sup>5</sup> and for the conclusions I am about to draw it does not much matter which limit we take. Probably 90 per cent. of the giant stars have masses between  $\frac{1}{2}$  and 5 times the Sun's, and we see that this is just the range in which radiation-pressure rises from unimportance to importance. It seems clear that a globe of gas of larger mass, in which radiation-pressure and gravitation are nearly balancing, would be likely to be unstable. The condition may not be strictly unstable in itself, but a small rotation or perturbation would make it so. It may therefore be conjectured that, if nebulous material began to concentrate into a mass much greater than 5 times the Sun's, it would probably break up, and continue to redivide until more stable masses resulted. Above the upper limit the chances of survival are small; when the lower limit is approached the danger has practically disappeared, and there is little likelihood of any further breaking-up. Thus the final masses are left distributed almost entirely between the limits given. To put the matter slightly differently, we are able to predict from general principles that the material of the stellar universe will aggregate primarily into masses chiefly lying between  $10^{33}$  and  $10^{34}$  grams; and this is just the magnitude of the masses of the stars according to astronomical observation.<sup>6</sup>

This study of the radiation and internal conditions of a star brings forward very pressingly a problem often debated in this Section: What is the source of the heat which the Sun and stars are continually squandering? The answer given is almost unanimous—that it is obtained from the gravitational energy converted as the star steadily contracts. But almost as unanimously this answer is ignored in its practical consequences. Lord Kelvin showed that this hypothesis, due to Helmholtz, necessarily dates the birth of the Sun about 20,000,000 years ago; and he made strenuous efforts to induce geologists and biologists to accommodate their demands to this time-scale. I do not think they proved altogether tractable. But it is among his own colleagues, physicists and astronomers, that the most outrageous violations of this limit have prevailed. I need only refer to Sir George Darwin's theory of the earth-moon system, to the present Lord Rayleigh's determination of the age of terrestrial rocks from occluded helium, and to all modern discussions of the statistical equilibrium of the stellar system. No one seems to have any hesitation, if it suits him, in carrying back the history of the earth long before the supposed date of formation of the solar system; and in some cases at least this appears to be justified

<sup>5</sup> As an illustration of these limits, iron has 26 outer electrons; if 10 break away the average molecular weight is 5; if 18 break away the molecular weight is 3. Eggert (*Phys. Zeits.*, 1919, p. 570) has suggested by thermodynamical reasoning that in most cases the two outer rings (16 electrons) would break away in the stars. The comparison of theory and observation for the dwarf stars also points to a molecular weight a little greater than 3.

<sup>6</sup> By admitting plausible assumptions closer limits could be drawn. Taking the molecular weight as 3.5, and assuming that the most critical condition is when  $\frac{1}{2}$  of gravitation is counterbalanced (by analogy with the case of rotating spheroids, in which centrifugal force opposes gravitation and creates instability), we find that the critical mass is just twice that of the Sun, and stellar masses may be expected to cluster closely round this value.

by experimental evidence which it is difficult to dispute. Lord Kelvin's date of the creation of the Sun is treated with no more respect than Archbishop Ussher's.

The serious consequences of this contraction hypothesis are particularly prominent in the case of giant stars, for the giants are prodigal with their heat and radiate at least a hundred times as fast as the Sun. The supply of energy which suffices to maintain the Sun for 10,000,000 years would be squandered by a giant star in less than 100,000 years. The whole evolution in the giant stage would have to be very rapid. In 18,000 years at the most a typical star must pass from the initial M stage to type G. In 80,000 years it has reached type A, near the top of the scale, and is about to start on the downward path. Even these figures are probably very much over-estimated.<sup>7</sup> Most of the naked-eye stars are still in the giant stage. Dare we believe that they were all formed within the last 80,000 years? The telescope reveals to us objects not only remote in distance but remote in time. We can turn it on a globular cluster and behold what was passing 20,000, 50,000, even 200,000 years ago—unfortunately not all in the same cluster, but different clusters representing different epochs of the past. As Shapley has pointed out, the verdict appears to be 'no change.' This is perhaps not conclusive, because it does not follow that individual stars have suffered no change in the interval; but it is difficult to resist the impression that the evolution of the stellar universe proceeds at a slow, majestic pace, with respect to which these periods of time are insignificant.

There is another line of astronomical evidence which appears to show more definitely that the evolution of the stars proceeds far more slowly than the contraction hypothesis allows; and perhaps it may ultimately enable us to measure the true rate of progress. There are certain stars, known as Cepheid variables, which undergo a regular fluctuation of light of a characteristic kind, generally with a period of a few days. This light change is *not* due to eclipse. Moreover, the colour quality of the light changes between maximum and minimum, evidently pointing to a periodic change in the physical condition of the star. Although these objects were formerly thought to be double stars, it now seems clear that this was a misinterpretation of the spectroscopic evidence. There is in fact no room for the hypothetical companion star; the orbit is so small that we should have to place it inside the principal star. Everything points to the period of the light pulsation being something intrinsic in the star; and the hypothesis advocated by Shapley, that it represents a mechanical pulsation of the star, seems to be the most plausible. I have already mentioned that the observed period does in fact agree with the calculated period of mechanical pulsation, so that the pulsation explanation survives one fairly stringent test. But whatever the cause of the variability, whether pulsation or rotation, provided only that it is intrinsic in the

<sup>7</sup> I have taken the ratio of specific heats at the extreme possible value,  $\frac{5}{3}$ ; that is to say, no allowance has been made for the energy needed for ionisation and internal vibrations of the atoms, which makes a further call on the scanty supply available.

star, and not forced from outside, the density must be the leading factor in determining the period. If the star is contracting, so that its density changes appreciably, the period cannot remain constant. Now, on the contraction hypothesis the change of density must amount to at least 1 per cent. in 40 years. (I give the figures for  $\delta$  Cephei, the best-known variable of this class.) The corresponding change of period should be very easily detectable. For  $\delta$  Cephei the period ought to decrease 40 seconds annually.

Now  $\delta$  Cephei has been under careful observation since 1785, and it is known that the change of period, if any, must be very small. S. Chandler found a decrease of period of  $\frac{1}{375}$  second per annum, and in a recent investigation E. Hertzsprung has found a decrease of  $\frac{1}{400}$  second per annum. The evidence that there is any decrease at all rests almost entirely on the earliest observations made before 1800, so that it is not very certain; but in any case the evolution is proceeding at not more than  $\frac{1}{4000}$  of the rate required by the contraction hypothesis. There must at this stage of the evolution of the star be some other source of energy which prolongs the life of the star 400-fold. The time-scale so enlarged would suffice for practically all reasonable demands.

I hope the dilemma is plain. Either we must admit that whilst the density changes 1 per cent. a certain period intrinsic in the star can change no more than  $\frac{1}{8000}$  of 1 per cent., or we must give up the contraction hypothesis.

If the contraction theory were proposed to-day as a novel hypothesis I do not think it would stand the smallest chance of acceptance. From all sides—biology, geology, physics, astronomy—it would be objected that the suggested source of energy was hopelessly inadequate to provide the heat spent during the necessary time of evolution; and, so far as it is possible to interpret observational evidence confidently, the theory would be held to be definitely negatived. Only the inertia of tradition keeps the contraction hypothesis alive—or rather, not alive, but an unburied corpse. But if we decide to inter the corpse, let us frankly recognise the position in which we are left. A star is drawing on some vast reservoir of energy by means unknown to us. This reservoir can scarcely be other than the sub-atomic energy which, it is known, exists abundantly in all matter; we sometimes dream that man will one day learn how to release it and use it for his service. The store is well-nigh inexhaustible, if only it could be tapped. There is sufficient in the Sun to maintain its output of heat for 15 billion years.

Certain physical investigations in the past year, which I hope we may hear about at this meeting, make it probable to my mind that some portion of this sub-atomic energy is actually being set free in the stars. F. W. Aston's experiments seem to leave no room for doubt that all the elements are constituted out of hydrogen atoms bound together with negative electrons. The nucleus of the helium atom, for example, consists of 4 hydrogen atoms bound with 2 electrons. But Aston has further shown conclusively that the mass of the helium atom is less than the sum of the masses of the 4 hydrogen atoms which enter into it; and in this at any rate the chemists agree with him. There is a loss of mass in the synthesis amounting to about 1 part in 120, the

atomic weight of hydrogen being 1.008 and that of helium just 4. I will not dwell on his beautiful proof of this, as you will no doubt be able to hear it from himself. Now mass cannot be annihilated, and the deficit can only represent the mass of the electrical energy set free in the transmutation. We can therefore at once calculate the quantity of energy liberated when helium is made out of hydrogen. If 5 per cent. of a star's mass consists initially of hydrogen atoms, which are gradually being combined to form more complex elements, the total heat liberated will more than suffice for our demands, and we need look no further for the source of a star's energy.

But is it possible to admit that such a transmutation is occurring? It is difficult to assert, but perhaps more difficult to deny, that this is going on. Sir Ernest Rutherford has recently been breaking down the atoms of oxygen and nitrogen, driving out an isotope of helium from them; and what is possible in the Cavendish laboratory may not be too difficult in the Sun. I think that the suspicion has been generally entertained that the stars are the crucibles in which the lighter atoms which abound in the nebulae are compounded into more complex elements. In the stars matter has its preliminary brewing to prepare the greater variety of elements which are needed for a world of life. The radio-active elements must have been formed at no very distant date; and their synthesis, unlike the generation of helium from hydrogen, is endothermic. If combinations requiring the addition of energy can occur in the stars, combinations which liberate energy ought not to be impossible.

We need not bind ourselves to the formation of helium from hydrogen as the sole reaction which supplies the energy, although it would seem that the further stages in building up the elements involve much less liberation, and sometimes even absorption, of energy. It is a question of accurate measurement of the deviations of atomic weights from integers, and up to the present hydrogen is the only element for which Mr. Aston has been able to detect the deviation. No doubt we shall learn more about the possibilities in due time. The position may be summarised in these terms: the atoms of all elements are built of hydrogen atoms bound together, and presumably have at one time been formed from hydrogen; the interior of a star seems as likely a place as any for the evolution to have occurred; whenever it did occur a great amount of energy must have been set free; in a star a vast quantity of energy is being set free which is hitherto unaccounted for. You may draw a conclusion if you like.

If, indeed, the sub-atomic energy in the stars is being freely used to maintain their great furnaces, it seems to bring a little nearer to fulfilment our dream of controlling this latent power for the well-being of the human race—or for its suicide.

So far as the immediate needs of astronomy are concerned, it is not of any great consequence whether in this suggestion we have actually laid a finger on the true source of the heat. It is sufficient if the discussion opens our eyes to the wider possibilities. We can get rid of the obsession that there is no other conceivable supply besides contraction, but we need not again cramp ourselves by adopting prematurely

what is perhaps a still wilder guess. Rather we should admit that the source is not certainly known, and seek for any possible astronomical evidence which may help to define its necessary character. One piece of evidence of this kind may be worth mentioning. It seems clear that it must be the high temperature inside the stars which determines the liberation of energy, as H. N. Russell has pointed out.<sup>8</sup> If so the supply may come mainly from the hottest region at the centre. I have already stated that the general uniformity of the opacity of the stars is much more easily intelligible if it depends on scattering rather than on true absorption; but it did not seem possible to reconcile the deduced stellar opacity with the theoretical scattering coefficient. Within reasonable limits it makes no great difference in our calculations at what parts of the star the heat energy is supplied, and it was assumed that it comes more or less evenly from all parts, as would be the case on the contraction theory. The possibility was scarcely contemplated that the energy is supplied entirely in a restricted region round the centre. Now, the more concentrated the supply, the lower is the opacity requisite to account for the observed radiation. I have not made any detailed calculations, but it seems possible that for a sufficiently concentrated source the deduced and the theoretical coefficients could be made to agree, and there does not seem to be any other way of accomplishing this. Conversely, we might perhaps argue that the present discrepancy of the coefficients shows that the energy supply is not spread out in the way required by the contraction hypothesis, but belongs to some new source only available at the hottest, central part of the star.

I should not be surprised if it is whispered that this address has at times verged on being a little bit speculative; perhaps some outspoken friend may bluntly say that it has been highly speculative from beginning to end. I wonder what is the touchstone by which we may test the legitimate development of scientific theory and reject the idly speculative. We all know of theories which the scientific mind instinctively rejects as fruitless guesses; but it is difficult to specify their exact defect or to supply a rule which will show us when we ourselves do err. It is often supposed that to speculate and to make hypotheses are the same thing; but more often they are opposed. It is when we let our thoughts stray outside venerable, but sometimes insecure, hypotheses that we are said to speculate. Hypothesis limits speculation. Moreover, distrust of speculation often serves as a cover for loose thinking; wild ideas take anchorage in our minds and influence our outlook; whilst it is considered too speculative to subject them to the scientific scrutiny which would exorcise them.

If we are not content with the dull accumulation of experimental facts, if we make any deductions or generalisations, if we seek for any theory to guide us, some degree of speculation cannot be avoided. Some will prefer to take the interpretation which seems to be most immediately indicated and at once adopt that as an hypothesis; others will rather seek to explore and classify the widest possibilities which are not definitely inconsistent with the facts. Either choice has its dangers;

<sup>8</sup> *Pub. Act. Soc. Pacific.* August 1919.

the first may be too narrow a view and lead progress into a cul-de-sac; the second may be so broad that it is useless as a guide, and diverges indefinitely from experimental knowledge. When this last case happens, it must be concluded that the knowledge is not yet ripe for theoretical treatment and speculation is premature. The time when speculative theory and observational research may profitably go hand in hand is when the possibilities, or at any rate the probabilities, can be narrowed down by experiment, and the theory can indicate the tests by which the remaining wrong paths may be blocked up one by one.

The mathematical physicist is in a position of peculiar difficulty. He may work out the behaviour of an ideal model of material with specifically defined properties, obeying mathematically exact laws, and so far his work is unimpeachable. It is no more speculative than the binomial theorem. But when he claims a serious interest for his toy, when he suggests that his model is like something going on in Nature, he inevitably begins to speculate. Is the actual body really like the ideal model? May not other unknown conditions intervene? He cannot be sure, but he cannot suppress the comparison; for it is by looking continually to Nature that he is guided in his choice of a subject. A common fault, to which he must often plead guilty, is to use for the comparison data over which the more experienced observer shakes his head; they are too insecure to build extensively upon. Yet even in this, theory may help observation by showing the kind of data which it is especially important to improve.

I think that the more idle kinds of speculation will be avoided if the investigation is conducted from the right point of view. When the properties of an ideal model have been worked out by rigorous mathematics, all the underlying assumptions being clearly understood, then it becomes possible to say that such and such properties and laws lead precisely to such and such effects. If any other disregarded factors are present, they should now betray themselves when a comparison is made with Nature. There is no need for disappointment at the failure of the model to give perfect agreement with observation; it has served its purpose, for it has distinguished what are the features of the actual phenomena which require new conditions for their explanation. A general preliminary agreement with observation is necessary, otherwise the model is hopeless; not that it is necessarily wrong so far as it goes, but it has evidently put the less essential properties foremost. We have been pulling at the wrong end of the tangle, which has to be unravelled by a different approach. But after a general agreement with observation is established, and the tangle begins to loosen, we should always make ready for the next knot. I suppose that the applied mathematician whose theory has just passed one still more stringent test by observation ought not to feel satisfaction, but rather disappointment—'Foiled again! This time I *had* hoped to find a discordance which would throw light on the points where my model could be improved.' Perhaps that is a counsel of perfection; I own that I have never felt very keenly a disappointment of this kind.

Our model of Nature should not be like a building—a handsome



structure for the populace to admire, until in the course of time someone takes away a corner stone and the edifice comes toppling down. It should be like an engine with movable parts. We need not fix the position of any one lever; that is to be adjusted from time to time as the latest observations indicate. The aim of the theorist is to know the train of wheels which the lever sets in motion—that binding of the parts which is the soul of the engine.

In ancient days two aviators procured to themselves wings. Dædalus flew safely through the middle air across the sea, and was duly honoured on his landing. Young Icarus soared upwards towards the Sun till the wax melted which bound his wings, and his flight ended in fiasco. In weighing their achievements perhaps there is something to be said for Icarus. The classic authorities tell us that he was only 'doing a stunt,' but I prefer to think of him as the man who certainly brought to light a constructional defect in the flying-machines of his day. So too in science. Cautious Dædalus will apply his theories where he feels most confident they will safely go; but by his excess of caution their hidden weaknesses cannot be brought to light. Icarus will strain his theories to the breaking-point till the weak joints gape. For a spectacular stunt? Perhaps partly; he is often very human. But if he is not yet destined to reach the Sun and solve for all time the riddle of its constitution, yet he may hope to learn from his journey some hints to build a better machine.

# British Association for the Advancement of Science.

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SECTION B : CARDIFF, 1920.

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## ADDRESS TO THE CHEMICAL SECTION BY

C. T. HEYCOCK, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

DURING its past eighty-nine years of useful life the British Association has, in the course of its evolution, established certain traditions; among these is the expectation that the sectional President shall deliver an address containing a summary of that branch of natural knowledge with which he has become especially acquainted.

The rapid accumulation of experimental observations during the last century, and the consequent necessity for classifying the observed facts with the aid of hypotheses and theories of ever-increasing complexity, make such summaries of knowledge essential, not only to the student of science, but also to the person of non-specialised education who desires to realise something of the tendencies and of the results of modern science.

At the present moment, when the whole world is in pause after having overcome the greatest peril which has ever threatened civilisation; when all productive effort, social, artistic, and scientific, is undergoing reorganisation preparatory to an advance which will eclipse in importance the progress made during the nineteenth century, such attempts to visualise the present condition of knowledge as are made in our Presidential Addresses are of particular value. It is, therefore, hardly necessary for me to apologise for an endeavour to place before you a statement upon the particular branch of science to which I have myself paid special attention; whatever faults may attend the mode of presentation, such a survey of a specific field of knowledge cannot but be of value to some amongst us.

I propose to deal to-day with the manner in which our present rather detailed knowledge of metallic alloys has been acquired, starting from the sparse information which was available thirty or forty years ago;

to show the pitfalls which have been avoided in the theoretical interpretation of the observed facts, and to sketch very briefly the present position of our knowledge.

The production of metals and their alloys undoubtedly constitutes the oldest of those chemical arts which ultimately expanded into the modern science of chemistry, with all its overwhelming mass of experimental detail and its intricate interweaving of theoretical interpretation of the observed facts. Tubal-Cain lived during the lifetime of our common ancestor, and was 'an instructor of every artificer in brass and iron'; and although it may be doubted whether the philologists have yet satisfactorily determined whether Tubal-Cain was really acquainted with the manufacture of such a complex metallic alloy as brass, it is certain that chemical science had its beginnings in the reduction of metals from their ores and in the preparation of useful alloys from those metals. In fact, metallic alloys, or mixtures of metals, have been used by mankind for the manufacture of implements of war and of agriculture, of coinage, statuary, cooking vessels, and the like from the very earliest times.

In the course of past ages an immense amount of practical information has been accumulated concerning methods of reducing metals, or mixtures of metals, from their ores, and by subsequent treatment, usually by heating and cooling, of adapting the resulting metallic product to the purpose for which it was required. Until quite recent times, however, the whole of this knowledge was entirely empirical in character, because it had no foundation in general theoretical principles; it was collected in haphazard fashion in accordance with that method of trial and error which led our forerunners surely, but with excessive expenditure of time and effort, to valuable results.

To-day I purpose dealing chiefly with the non-ferrous alloys, not because any essential difference in type exists between the ferrous and non-ferrous alloys, but merely because the whole field presented by the chemistry of the metals and their alloys is too vast to be covered in any reasonable length of time.

The earliest recorded scientific investigations on alloys were made in 1722 by Reaumur, who employed the microscope to examine the fractured surfaces of white and grey cast iron and steel.

In 1808 Widmanstätten cut sections from meteorites, which he polished and etched.

The founder, however, of modern metallography is undoubtedly H. C. Sorby, of Sheffield. Sorby's early petrographic work on the examination of thin sections of rock under the microscope led him to a study of meteorites and of iron and steel, and in a paper read before the British Association in 1864 he describes briefly (I quote his own words) how sections 'of iron and steel may be prepared for the microscope so as to exhibit their structure to a perfection that leaves little to be desired. They show various mixtures of iron, and two or three well-defined compounds of iron and carbon, graphite, and slag; these constituents being present in different proportions and arranged in various manners, give rise to a large number of varieties of iron and steel, differing by well-marked and very striking peculiarities

of structure.' The methods described by Sorby for polishing and etching alloys and his method of vertical illumination (afterwards improved by Beck) are employed to-day by all who work at this branch of metallography.

The lantern-slides, now shown, were reproduced from his original photographs; they form a lasting memorial to his skill as an investigator and his ability as a manipulator. In 1887 Dr. Sorby published a paper on the microscopical structure of iron and steel in the *Journal of the Iron and Steel Institute*. This masterpiece of clear writing and expression, even with our present knowledge, needs but little emendation. In this paper he describes Free Iron (ferrite) carbon as graphite, the pearly constituent as a very fine laminar structure (pearlitic structure), combined iron as the chief constituent of white cast iron (cementite), slag inclusions, effect of tempering steel, effect of working iron and steel, cementation of wrought iron, and the decarbonisation of cast iron by haematite. A truly remarkable achievement for one man.

From 1854-68 Mattheisen published in the Reports of the British Association and in the Proceedings and Transactions of the Royal Society, a large number of papers on the electrical conductivity, tenacity, and specific gravity of pure metals and alloys. He concluded that alloys are either mixtures of definite chemical compounds with an excess of one or other metal, or solutions of the definite alloy in the excess of one of the metals employed, forming, in their solid condition, what he called a solidified solution. This idea of a solidified solution has developed into a most fruitful theory upon which much of our modern notions of alloys depends. Although, at the time, the experiments on the electrical conductivity did not lead to very definite conclusions, the method has since been used with great success in testing for the presence of minute quantities of impurities in the copper used for conductors.

In the *Philosophical Magazine* for 1875, F. Guthrie, in a remarkable paper, quite unconnected with alloys, gave an account of his experiments on salt solutions and attached water. He was led to undertake this work by a consideration of a paper by Dr. J. Rea, the Arctic explorer, on the comparative saltiness of freshly formed and of older ice floes. Guthrie showed that the freezing-point of solutions was continuously diminished as the percentage of common salt increased, and that this lowering increased up to 23.6 per cent. of salt, when the solution solidified as a whole at about 22° C. He further showed, and this is of great importance, that the substance which first separated from solutions more dilute than 23.6 was pure ice. To the substance which froze as a whole, giving crystals of the same composition as the mother liquor, he gave the name cryohydrate. At the time he thought that the cryohydrate of salt containing 23.6 per cent. NaCl and 76.4 per cent. of water was a chemical compound  $2\text{NaCl} \cdot 21\text{H}_2\text{O}$ . In succeeding years he showed that a large number of other salts gave solutions which behaved in a similar manner to common salt. He abandoned the idea that the cryohydrates were chemical compounds.

How clear his views were will be seen by quotations from his

paper in the *Phil. Mag.* (5) I. and II., 1876, in which he states: (i.) When a solution weaker than the cryohydrate loses heat, ice is formed. (ii.) Ice continues to form and the temperature to fall until the cryohydrate is reached. (iii.) At the point of saturation ice and salt separate simultaneously and the solid and liquid portions are identical in composition.

These results can be expressed in the form of a simple diagram as shown in the slide.

In a subsequent paper, *Phil. Mag.* (5) 17, he extends his experiments to solvents other than water, and states that the substances which separate at the lowest temperature are neither atomic nor molecular; this lowest melting-point mixture of two bodies he names the eutectic mixture. In the same paper he details the methods of obtaining various eutectic alloys of bismuth, lead, tin, and cadmium.

We have, in these papers of Guthrie's, the first important clue to what occurs on cooling a fused mixture of metals. The researches of Sorby and Guthrie, undertaken as they were for the sake of investigating natural phenomena, are a remarkable example of how purely scientific experiment can lead to most important practical results. It is not too much to claim for these investigators the honour of being the originators of all our modern ideas of metallurgy. Although much valuable information had been accumulated, no rapid advance could be made until some general theory of solution had been developed. In 1878 Raoult first began his work on the depression of the freezing-point of solvents due to the addition of dissolved substances, and he continued, at frequent intervals, to publish the results of his experiments up to the time of his death in 1901. He established for organic solvents certain general laws: (i.) that for moderate concentrations the fall of the freezing-point is proportional to the weight of the dissolved substance present in a constant weight of solvent; (ii.) that when the falls produced in the same solvent by different dissolved substances are compared, it is found that a molecular weight of a dissolved substance produces the same fall of the freezing-point, whatever the substance is. When, however, he applied the general laws which he had established for organic solvents to aqueous solutions of inorganic acids, bases, and salts, the results obtained were hopelessly discrepant. In a paper in the *Zeit. Physik. Chem.* for 1888 on 'Osmotic Pressure in the analogy between solutions and gases, Van't Hoff showed that the experiments of Pfeffer on osmotic pressure could be explained on the theory that dissolved substances were, at any rate for dilute solutions, in a condition similar to that of a gas; that they obeyed the laws of Boyle, Charles, and Avogadro, and that on this assumption the depression of the freezing-point of a solvent could be calculated by means of a simple formula. He also showed that the exceptions which occurred to Raoult's laws, when applied to aqueous solutions of electrolytes, could be explained by the assumption, first made by Arrhenius, that these latter in solution are partly dissociated into their ions. The result of all this work was to establish a general theory applicable to all solutions which has been widespread in its applications. It is true that Van't Hoff's theory has been violently attacked;

but it enables us to calculate the depression of the freezing-points of a large number of solvents. To do this it is necessary to know the latent heat of fusion of the pure solvent and the absolute temperature of the freezing-point of the solution. That the numbers calculated are in very close accord with the experimental values constitutes a strong argument in favour of the theory. From this time the study of alloys began to make rapid progress. Laurie (*Chem. Soc. Jour.* 1888), by measuring the potential difference of voltaic cells composed of plates of alloy and the more negative element immersed in a solution of a salt of one of the component metals, obtained evidence of the existence of compounds such as  $\text{CuZn}_3$ ,  $\text{Cu}_3\text{Sn}$ . In 1889 F. H. Neville and I, whilst repeating Raoult's experiments on the lowering of the freezing-point of organic solvents, thought that it was possible that the well-known fact that alloys often freeze at a lower temperature than either of their constituents might be explained in a similar way. In a preliminary note communicated to the Chemical Society on March 21, 1889, on the same evening that Professor Ramsay read his paper on the molecular weights of metals as determined by the depression of the vapour pressure, we showed that the fall produced in the freezing-point of tin by dissolving metals in it was for dilute solutions directly proportional to the concentration. We also showed that the fall produced in the freezing-point of tin by the solution of one atomic weight of metal in 100 atomic weights of tin was a constant.

G. Tannman about the same time (*Zeit. Physikal. Chemie*, III., 44, 1889) arrived at a similar conclusion, using mercury as a solvent.

These experiments helped to establish the similarity between the behaviour of metallic solutions or alloys and that of aqueous and other solutions of organic compounds in organic solvents. That our experiments were correct seemed probable from the agreement between the observed depression of the freezing-point and the value calculated from Van't Hoff's formula for the case of those few metals whose latent heats of fusion had been determined with any approach to accuracy.

Our experiments, subsequently extended to other solvents, led to the conclusion that in the case of most metals dissolved in tin the molecular weight is identical with the atomic weight; in other words, that the metals in solution are monatomic. This conclusion, however, involves certain assumptions. Prof. Ramsay's experiments on the lowering of the vapour pressure of certain amalgams point to a similar conclusion.

So far our work had been carried out with mercury thermometers, standardised against a platinum resistance pyrometer, but it was evident that, if it was to be continued, we must have some method of extending our experiments to alloys which freeze at high temperatures. The thermo couple was not at this stage a reliable instrument; fortunately, however, Callendar and Griffiths had brought to great perfection the electrical resistance pyrometer (*Phil. Trans. A*, 1887 and 1891.) Dr. E. H. Griffiths kindly came to our aid, and with his help we installed a complete electrical resistance set. As at this time the freezing-points of pure substances above  $300^\circ$  were not known with any degree of accuracy, we began by making these measurements:—

*Table of Freezing-points.*

—	Carnelly's Tables	Holborn & Wien, 1892	Callendar & Griffiths, 1892	Neville & Heycock, 1895	Burgess & Le Chatelier, 1912. High Tem- perature Measure- ments
Tin . . . .	—	—	231·7	231·9	231·9
Zinc . . . .	433	—	417·6	419·0	419·4
Lead . . . .	—	—	—	327·6	327·4
Antimony . . . .	432	—	—	629·5	630·7 & 629·2
Magnesium . . . .	—	—	—	1632·6	650
Aluminium . . . .	700	—	—	1654·5	658
Silver . . . .	954	968	972	960·7	960·9
Gold . . . .	1,045	1,072	1,037	1,061·7	1,062·4
Copper . . . .	1,054	1,082	—	1,080·5	1,083
Sulphur B.P. . . .	448	—	444·53	—	444·7

<sup>1</sup> Contaminated with silicon.<sup>2</sup> Known to be impure.

With the exception of silver and gold, these metals were the purest obtainable in commerce.

Two facts are evident from the consideration of this table: (a) the remarkable accuracy of Callendar's formula connecting the Temperature Centigrade with the change of resistance of a pure platinum wire; (b) the accuracy of Callendar and Griffith's determination of the boiling-point of sulphur. Although the platinum resistance pyrometer had at this time only been compared with the air thermometer up to 600° C., it will be noted that the extrapolation from 600° to nearly 1100 was justified.

I cannot leave the subject of high-temperature measurements without referring to the specially valuable work of Burgess, and also to Eza Griffiths' book on high-temperature measurements, which contains an excellent summary of the present state of our knowledge of this important subject.

During the period that the above work on non-ferrous alloys was being done, great progress was being made in the study of iron and steel by Osmond and Le Chatelier. In 1890 the Institute of Mechanical Engineers, not apparently without considerable misgivings on the part of some of its members, formed an Alloys Research Committee. This Committee invited Professor (afterwards Sir William) Roberts-Austen to undertake research work for them. The results of his investigations are contained in a series of five valuable Reports extending from 1891 to 1899, published in the Journal of the Institute. The first report contained a description of an improved form of the Le Chatelier recording pyrometer, and the instrument has since proved a powerful weapon of research. In the second report, issued in 1893, the effects on the properties of copper of small quantities of arsenic, bismuth, and antimony were discussed. Whilst some engineers advocated, others as strongly controverted, the beneficial results of small quantities of

arsenic on the copper used for the fireboxes of locomotives. The report showed that the presence of from 5.1 per cent. of arsenic was highly beneficial. The third report dealt with electric welding and the production of alloys of iron and aluminium. The fourth report is particularly valuable, as it contains a *résumé* of the Bakerian Lecture given by Roberts-Austen on the diffusion of metals in the solid state, in which he showed that gold, even at as low a temperature as 100°, could penetrate into lead, and that iron became carbonised at a low red heat by contact with a diamond in a vacuum. In 1899 the fifth report appeared on the effects of the addition of carbon to iron. This report is of especial importance, because, besides a description of the thermal effects produced by carbon, which he carefully plotted and photographed, he described the microscopical appearance of the various constituents of iron. The materials of this report, together with the work of Osmond and others on steel and iron, provided much of the material on which Professor Bakhuis Roozeboom founded the iron carbon equilibrium diagram. Reference should also be made to the very valuable paper by Stansfield on the present position of the solution theory of carbonised iron (*Journ. Iron and Steel Inst.*, 11, 1900, p. 317). It may be said of this fifth report, and the two papers just referred to, that they form the most important contribution to the study of iron and steel that has ever been published. Although the diagram for the equilibrium of iron and carbon does not represent the whole of the facts, it affords the most important clue to these alloys, and undoubtedly forms the basis of most of the modern practice of steel manufacture. (Slide showing iron carbon diagram.)

Many workers, both at home and abroad, were now actively engaged in metallurgical work—Stead, Osmond, Le Chatelier, Arnold, Hadfield, Carpenter, Ewing, Rosenhain, and others too numerous to mention.

In 1897 Neville and I determined the complete freezing-point curve of the copper-tin alloys, confirming and extending the work of Roberts-Austen, Stansfield, and Le Chatelier; but the real meaning of the curve remained as much of a mystery as ever. Early in 1900 Sir G. Stokes suggested to us that we should make a microscopic examination of a few bronzes as an aid to the interpretation of the singularities of the freezing-point curve. An account of this work, which occupied us for more than two years, was published as the Bakerian Lecture of the Royal Society in February 1903. Whilst preparing a number of copper-tin alloys of known composition we were struck by the fact that the crystalline pattern which developed on the free surface of the slowly cooled alloys was entirely unlike the structure developed by polishing and etching sections cut from the interior; it therefore appeared probable that changes were going on within the alloys as they cooled. In the hope that, as Sorby had shown in the case of steel, we could stereotype or fix the change by sudden cooling, we melted small ingots of the copper-tin alloys and slowly cooled them to selected temperatures and then suddenly chilled them in water. The results of this treatment were communicated to the Royal Society and published in the Proceedings, February 1901. (Slides showing effects of chilling alloys.)



To apply this method to a selected alloy we first determined its cooling curve by means of an automatic recorder, the curve usually showing several halts or steps in it. The temperature of the highest of these steps corresponded with a point on the liquidus, *i.e.*, when solid first separated out from the molten mass. To ascertain what occurred at the subsequent halts, ingots of the melted alloy were slowly cooled to within a few degrees above and below the halt and then chilled, with the result just seen on the screen.

The method of chilling also enabled us to fix, with some degree of accuracy, the position of points on the solidus. If an alloy, chilled when it is partly solid and partly liquid, is polished and etched, it will be seen to consist of large primary combs embedded in a matrix consisting of mother liquor, in which are disseminated numerous small combs, which we called 'chilled primary.' By repeating the process at successively lower and lower temperatures we obtained a point at which the chilled primary no longer formed, *i.e.*, the upper limit of the solidus.

Although we made but few determinations of the physical properties of the alloys, it is needless to say how much they vary with the temperature and with the rapidity with which they are heated or cooled.

From a consideration of the singularities in the liquidus curve, coupled with the microscopic examination of slowly cooled and chilled alloys, we were able to divide the copper-tin alloys into certain groups having special qualities. It would take far too long to discuss these divisions. In interpreting our result we were greatly assisted not only by the application of the phase rule, but also by the application of Roozeboom's theory of solid solution (unfortunately Professor Roozeboom's letters were destroyed by fire in June 1910) and by the advice he kindly gave us. At the time the paper was published we expressly stated that we did not regard all our results as final, as much more work was required to clear up points still obscure. Other workers—Shepherd and Blough, Giolitti and Tavanti—have somewhat modified the diagram. (Slides shown.)

Neither Shepherd and Blough nor Hoyt have published the photomicrographs upon which their results are based, so that it is impossible to criticise their conclusions. Giolitti and Tavanti have published some microphotographs, from which it seems that they had not allowed sufficient time for equilibrium to be established. In this connection I must call attention to the excellent work of Haughton on the constitution of the alloys of copper and tin (*Journ. Institute of Metals*, March 1915). He investigated the alloys rich in tin, and illustrated his conclusions by singularly beautiful microphotographs, and has done much to clear up doubtful points in this region of the diagram. I have dwelt at some length on this work, for copper-tin is probably the first of the binary alloys on which an attempt had been made to determine the changes which take place in passing from one pure constituent to the other. I would again call attention to the fact that without a working theory of solution the interpretation of the results would have been impossible.

Since 1900, many complete equilibrium diagrams have been pub-

lished; amongst them may be mentioned the work of Rosenhain and Tucker on the lead-tin alloys (*Phil. Trans.*, 1908), in which they describe hitherto unsuspected changes on the lead rich side which go on when these alloys are at quite low temperatures, also the constitution of the alloys of aluminium and zinc; the work of Rosenhain and Archbutt (*Phil. Trans.*, 1911), and quite recently the excellent work of Vivian, on the alloys of tin and phosphorus, which has thrown an entirely new light on this difficult subject.

So far I have called attention to some of the difficulties encountered in the examination of binary alloys. When we come to ternary alloys the difficulties of carrying out an investigation are enormously increased, whilst with quaternary alloys they seem almost insurmountable; in the case of steels containing always six, and usually more, constituents, we can only hope to get information by purely empirical methods.

Large numbers of the elements and their compounds which originally were laboriously prepared and investigated in the laboratory and remained dormant as chemical curiosities for many years have, in the fulness of time, taken their places as important and, indeed, essential articles of commerce. Passing over the difficulties encountered by Davy in the preparation of metallic sodium and by Faraday in the production of benzene (both of which materials are manufactured in enormous quantities at the present time), I may remark that even during my own lifetime I have seen a vast number of substances transferred from the category of rare laboratory products to that which comprises materials of the utmost importance to the modern metallurgical industries. A few decades ago, aluminium, chromium, cerium, thorium, tungsten, manganese, magnesium, molybdenum, nickel, calcium and calcium carbide, carborundum, and acetylene, were unknown outside the chemical laboratory of the purely scientific investigator; to-day these elements, their compounds and alloys, are amongst the most valuable of our industrial metallic products. They are essential in the manufacture of high-speed steels, of armour plate, of filaments for the electric bulb lamp, of incandescent gas mantles, and of countless other products of modern scientific industry.

All these metallic elements and compounds were discovered, and their industrial uses foreshadowed, during the course of the purely academic research work carried out in our Universities and Colleges; all have become the materials upon which great and lucrative industries have been built up. Although the scientific worker has certainly not exhibited any cupidity in the past—although he has been content to rejoice in his own contributions to knowledge, and to see great manufacturing enterprises founded upon his work—it is clear that the obligation devolves upon those who have reaped in the world's markets the fruit of scientific discovery to provide from their harvest the financial aid without which scientific research cannot be continued.

The truth of this statement is well understood by those of our great industrial leaders who are engaged in translating the results of scientific research into technical practice. As evidence of this I may quote the magnificent donation of 210,000*l.* by the British Oil Companies towards the endowment of the school of Chemistry in the University of Cam-

bridge, the noble bequest of the late Dr. Messel, one of the most enlightened of our technical chemists, for defraying the cost of scientific research, the gifts of the late Dr. Ludwig Mond towards the upkeep and expansion of the Royal Institution, one of the strongholds of British chemical research, and the financial support given by the Goldsmiths' and others of the great City of London Livery Companies (initiated largely by the late Sir Frederick Abel, Sir Frederick Bramwell, and Mr. George Matthey), to the foundation of the Imperial College of Science and Technology. The men who initiated these gifts have been themselves intimately associated with developments both in science and industry; they have understood that the field must be prepared before the crop can be reaped. Fortunately our great chemical industries are, for the most part, controlled and administered by men fully conversant with the mode in which technical progress and prosperity follow upon scientific achievement; and it is my pleasant duty to record that within the last few weeks the directors of one of our greatest chemical-manufacturing concerns have, with the consent of their shareholders, devoted £100,000 to research. Doubtless other chemical industries will in due course realise what they have to gain by an adequate appreciation of pure science.

If the effort now being made to establish a comprehensive scheme for the resuscitation of chemical industry within our Empire is to succeed, financial support on a very liberal scale must be forthcoming, from the industry itself, for the advancement of purely scientific research. This question has been treated recently in so able a fashion by Lord Moulton that nothing now remains but to await the results of his appeal for funds in aid of the advancement of pure science.

In order to prevent disappointment, and a possible reaction in the future, in those who endow pure research, it is necessary to give a word of warning. It must be remembered that the history of science abounds in illustrations of discoveries, regarded at the time as trivial which have in after years become epoch-making.

In illustration I would cite Faraday's discovery of electro-magnetic induction. He found that when a bar magnet was thrust into the core of a bobbin of insulated copper wire, whose terminals were connected with a galvanometer, a momentary current was produced; whilst on withdrawing the magnet a momentary reverse current occurred; a purely scientific experiment destined in later years to develop into the dynamo and with it the whole electrical industry. Another illustration may be given: Guyton de Morveau, Northmore, Davy, Faraday and Cagniard Latour between 1800 and 1850 were engaged in liquefying many of the gases. Hydrogen, oxygen, nitrogen, marsh gas, carbon-monoxide, and nitric oxide, however, resisted all efforts, until the work of Joule and Andrews gave the clue to the causes of failure. Some thirty years later by careful application of the theoretical considerations all the gases were liquefied. The liquefaction of oxygen and nitrogen now forms the basis of a very large and important industry.

Such cases can be multiplied indefinitely in all branches of science.

Perhaps the most pressing need of the present day lies in the cultivation of a better understanding between our great masters of productive industry, the shareholders to whom they are in the first degree responsible, and our scientific workers; if, by reason of any turbidity of vision, our large manufacturing corporations fail to discern that, in their own interest, the financial support of purely scientific research should be one of their first cares, technical advance will slacken and other nations, adopting a more far-sighted policy, will forge ahead in science and technology. It should, I venture to think, be the bounden duty of everyone who has at heart the aims and objects of the British Association to preach the doctrine that in closer sympathy between all classes of productive labour, manual and intellectual, lies our only hope for the future. I cannot do better than conclude by quoting the words of Pope, one of our most characteristically British poets:

‘ By mutual confidence and mutual aid  
Great deeds are done and great discoveries made ’



# British Association for the Advancement of Science.

SECTION C: CARDIFF

## ADDRESS TO THE GEOLOGICAL SECTION

BY

FRANCIS ARTHUR BATHER, M.A., D.Sc., F.R.S.,

PRESIDENT OF THE SECTION.

### *FOSSILS AND LIFE.*

OF the many distinguished men who have preceded me in this chair only eight can be described as essentially palaeontologists; and among them few seized the occasion to expound the broader principles of their science. I propose, then, to consider the Relations of Palaeontology to the other Natural Sciences, especially the Biological, to discuss its particular contribution to biological thought, and to inquire whether its facts justify certain hypotheses frequently put forward in its name. Several of those hypotheses were presented to you in his usual masterly manner by Dr. Smith Woodward in 1909, and yet others are clearly elucidated in two Introductions to Palaeontology which we have recently delighted to welcome as British products: the books by Dr. Morley Davies and Dr. H. L. Hawkins. If I subject those attractive speculations to cold analysis it is from no want of admiration or even sympathy, for in younger days I too have sported with Vitalism in the shade and been caught in the tangles of Transcendental hair.

### *The Differentia of Palaeontology.*

Like Botany and Zoology, Palaeontology describes the external and internal form and structure of animals and plants; and on this description it bases, first, a systematic classification of its material; secondly, those broader inductions of comparative anatomy which constitute morphology, or the science of form. Arising out of these studies are the questions of relation—real or apparent kinship, lines of descent, the how and the why of evolution—the answers to which reflect their light back on our morphological and classificatory systems. By a different approach we map the geographical distribution of genera and species, thus helping to elucidate changes of land and sea, and so barring out one hypothesis of racial descent or unlocking the door to another. Again, we study collective faunas and floras, unravelling the interplay of their component animals and plants, or inferring from each assemblage the climatic and other physical agents that favoured, selected, and delimited it.

All this, it may be said, is nothing more than the Botany and Zoology of the past. True, the general absence of any soft tissues, and the obscured or fragmentary condition of those harder parts which alone are preserved, make the studies of the palaeontologist more difficult, and drive him to special methods. But the result is less complete: in short, an inferior and unattractive branch of Biology. Let us relegate it to Section C!

Certainly the relation of Palaeontology to Geology is obvious. It is a part of that general history of the Earth which is Geology. And it is an essential part even of physical geology, for without life not merely would our series of strata have lacked the coal measures, the mountain limestones, the chalks, and the siliceous earths, but the changes of land and sea would have been far other. To the scientific interpreter of Earth-history, the importance of fossils lies first in their value as date-markers; secondly, in the light which they cast on barriers and currents, on seasonal and climatic variation. Conversely, the history of life has itself been influenced by geologic change. But all this is just as true of the present inhabitants of the globe as it is of their predecessors. It does not give the *differentia* of Palaeontology.

That which above all distinguishes Palaeontology—the study of ancient creatures, from Neontology—the study of creatures now living, that which raises it above the mere description of extinct assemblages of life-forms, is the concept of Time. Not the quasi-absolute time of the clock, or rather of the sun; not various unrelated durations; but an orderly and related succession, coextensive, in theory at least, with the whole history of life on this planet. The bearing of this obvious statement will appear from one or two simple illustrations.

### *Effect of the Time-concept on Principles of Classification.*

Adopting the well-tried metaphor, let us imagine the tree of life buried, except for its topmost twigs, beneath a sand-dune. The neontologist sees only the unburied twigs. He recognises certain rough groupings, and constructs a classification accordingly. From various hints he may shrewdly infer that some twigs come from one branch, some from another; but the relations of the branches to the main stem are matters of speculation, and when branches have become so interlaced that their twigs have long been subjected to the same external influences, he will probably be led to incorrect conclusions. The palaeontologist then comes, shovels away the sand, and by degrees exposes the true relations of branches and twigs. His work is not yet accomplished, and probably he never will reveal the root and lower part of the tree; but already he has corrected many natural, if not inevitable, errors of the neontologist.

I could easily occupy the rest of this hour by discussing the profound changes wrought by this conception on our classification. It is not that Orders and Classes hitherto unknown have been discovered, not that some erroneous allocations have been corrected, but the whole basis of our system is being shifted. So long as we were dealing with

a horizontal section across the tree of life—that is to say, with an assemblage of approximately contemporaneous forms—or even with a number of such horizontal sections, so long were we confined to simple description. Any attempt to frame a causal connection was bound to be speculative. Certain relations of structure, as of cloven hooves with horns and with a ruminant stomach, were observed, but, as Cuvier himself insisted, the laws based on such facts were purely empirical. Huxley, then, was justified in maintaining, as he did in 1863 and for long after, that a zoological classification could be based with profit on ‘purely structural considerations’ alone. ‘Every group in that [kind of] classification is such in virtue of certain structural characters, which are not only common to the members of that group, but distinguish it from all others; and the statement of these constitutes the definition of the group.’ In such a classification the groups or categories—from species and genera up to phyla—are the expressions of an arbitrary intellectual decision. From Linnaeus downwards botanists and zoologists have sought for a classification that should be not arbitrary but natural, though what they meant by ‘natural’ neither Linnaeus nor his successors either could or would say. Not, that is, until the doctrine of descent was firmly established, and even now its application remains impracticable, except in those cases where sufficient proof of genetic connection has been furnished—as it has been mainly by palaeontology. In many cases we now perceive the causal connection; and we recognise that our groupings, so far as they follow the blood-red clue, are not arbitrary but tables of natural affinity.

Fresh difficulties, however, arise. Consider the branching of a tree. It is easy to distinguish the twigs and the branches each from each, but where are we to draw the line along each ascending stem? To convey the new conception of change in time we must introduce a new set of systematic categories, called grades or series, keeping our old categories of families, orders, and the like for the vertical divisions between the branches. Thus, many crinoids with pinnulate arms arose from others in which the arms were non-pinnulate. We cannot place them in an Order by themselves, because the ancestors belonged to two or three Orders. We must keep them in the same Orders as their respective ancestors, but distinguish a Grade Pinnata from a Grade Impinnata.

This sounds fairly simple, and for the larger groups so it is. But when we consider the genus, we are met with the difficulty that many of our existing genera represent grades of structure affecting a number of species, and several of those species can be traced back through previous grades. This has long been recognised, but I take a modern instance from H. F. Oshorn’s ‘Equidae’ (1918, Mem. Amer. Mus. N.H., n.s. II. 51): ‘The line between such species as *Miohippus* (*Mesohippus*) *meteuulophus* and *M. brachystylus* of the Leptauchenia zone and *M. (Mesohippus) intermedius* of the Protoceras zone is purely arbitrary. It is obvious that members of more than one phylum [*i.e.*, lineage] are passing from one genus into the next, and *Mesohippus* *meteuulophus* and *M. brachystylus* may with equal consistency be referred to *Miohippus*.’



The problem is reduced to its simplest elements in the following scheme:—

<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>	<i>f</i>	Italics.
A	B	C	D	E	F	Lower-case Romans
A	B	C	D	E	F	Capital Romans.
$\alpha$	$\beta$	$\gamma$	$\delta$	$\epsilon$	$\phi$	Greek.

Our genera are equivalent to the forms of letters: Italics, Roman, Greek, and so forth. The successive species are the letters themselves. Are we to make each species a genus? Or would it not be better to confess that here, as in the case of many larger groups, our basis of classification is wrong? For the palaeontologist, at any rate, the lineage *a*, A,  $\alpha$ ,  $\alpha$ , is the all-important concept. Between these forms he finds every gradation; but between *a* and *b* he perceives no connection.

In the old classification the vertical divisions either were arbitrary, or were gaps due to ignorance. We are gradually substituting a classification in which the vertical divisions are based on knowledge, and the horizontal divisions, though in some degree arbitrary, often coincide with relatively sudden or physiologically important changes of form.

This brings us to the last point of contrast. Our definitions can no longer have the rigid character emphasised by Huxley. They are no longer purely descriptive. When it devolved on me to draw up a definition of the great group Echinodermata, a definition that should include all the fossils, I found that scarcely a character given in the textbooks could certainly be predicated of every member of the group. The answer to the question, 'What is an Echinoderm?' (and you may substitute Mollusc, or Vertebrate, or what name you please) has to be of this nature: An Echinoderm is an animal descended from an ancestor possessed of such-and-such characters differentiating it from other animal forms, and it still retains the imprint of that ancestor, though modified and obscured in various ways according to the class, order, family, and genus to which it belongs. The definitions given by Professor Charles Schuchert in his classification of the Brachiopoda (1913, Eastman's 'Zittel') represent an interesting attempt to put these principles into practice. The Family Porambonitidae, for instance, is thus defined: 'Derived (out of Syntrophiidae), progressive, semi-rostrate Pentamerids, with the deltidia and chlidia vanishing more and more in time. Spondylia and cruralia present, but the former tends to thicken and unite with the ventral valve.'

The old form of diagnosis was *per genus et differentiam*. The new form is *per proavum et modificationem*.

Even the conception of our fundamental unit, the species, is insecure owing to the discovery of gradual changes. But this is a difficulty which the palaeontologist shares with the neontologist.

Let us consider another way in which the time-concept has affected biology.

#### *Effect of the Time-concept on Ideas of Relationship.*

Etienne Geoffroy-Saint Hilaire was the first to compare the embryonic stages of certain animals with the adult stages of animals considered

interior. Through the more precise observations of Von Baer, Louis Agassiz, and others, the idea grew until it was crystallised by the poetic imagination of Haeckel in his fundamental law of the reproduction of life—namely, that every creature tends in the course of its individual development to pass through stages similar to those passed through in the history of its race. This principle is of value if applied with the necessary safeguards. If it was ever brought into disrepute, it was owing to the reckless enthusiasm of some embryologists, who unwarrantably extended the statement to all shapes and structures observed in the developing animal, such as those evoked by special conditions of larval existence, sometimes forgetting that every conceivable ancestor must at least have been capable of earning its own livelihood. Or, again, they compared the early stages of an individual with the adult structure of its contemporaries instead of with that of its predecessors in time. Often, too, the searcher into the embryology of creatures now living was forced to study some form that really was highly specialised, such as the unstalked Crinoid *Antedon*, and he made matters worse by comparing its larvae with forms far too remote in time. Allman, for instance, thought he saw in the developing *Antedon* a Cystid stage, and so the Cystids were regarded as the ancestors of the Crinoids; but we now find that stage more closely paralleled in some Crinoids of Carboniferous and Permian age, and we realise that the Cystid structure is quite different.

Such errors were due to the ignoring of time relations or to lack of acquaintance with extinct forms, and were beautifully illustrated in those phylogenetic trees which, in the 'eighties, every dissector of a new or striking animal thought it his duty to plant at the end of his paper. The trees have withered, because they were not rooted in the past.

A similar mistake was made by the palaeontologist who, happening on a new fossil, blazoned it forth as a link between groups previously unconnected—and in too many cases unconnected still. This action, natural and even justifiable under the old purely descriptive system, became fallacious when descent was taken as the basis. In those days one heard much of generalised types, especially among the older fossils; animals were supposed to combine the features of two or three classes. This mode of thought is not quite extinct, for in the last American edition of Zittel's 'Palaeontology' *Stephanocrinus* is still spoken of as a Crinoid related to the Blastoids, if not also to the Cystids. Let it be clear that these so-called 'generalised' or 'annectant' types are not regarded by their expositors as ancestral. Of course, a genus existing at a certain period may give rise to two different genera of a succeeding period, as possibly the Devonian *Coelocrinus* evolved into *Agaricocrinus*, with concave base, and into *Dorycrinus*, with convex base, both Carboniferous genera. But, to exemplify the kind of statement here criticised, perhaps I may quote from another distinguished writer of the present century: 'The new genus is a truly annectant form uniting the Melocrinidae and the Platycrinidae.' Now the genus in question appeared, so far as we know, rather late in the Lower Carboniferous, whereas both Platycrinidae and Melocrinidae were already

established in Middle Silurian time. How is it possible that the far later form should unite these two ancient families? Even a *mésalliance* is inconceivable. In a word, to describe any such forms as 'annectant' is not merely to misinterpret structure but to ignore time.

As bold suggestions calling for subsequent proof these speculations had their value, and they may be forgiven in the neontologist, if not in the palaeontologist, if we regard them as erratic pioneer tracks blazed through a tangled forest. As our acquaintance with fossils enlarged, the general direction became clearer, and certain paths were seen to be impossible. In 1881, addressing this Association at York, Huxley could say: 'Fifty years hence, whoever undertakes to record the progress of palaeontology will note the present time as the epoch in which the law of succession of the forms of the higher animals was determined by the observation of palaeontological facts. He will point out that, just as Steno and as Cuvier were enabled from their knowledge of the empirical laws of co-existence of the parts of animals to conclude from a part to a whole, so the knowledge of the law of succession of forms empowered their successors to conclude, from one or two terms of such a succession, to the whole series, and thus to divine the existence of forms of life, of which, perhaps, no trace remains, at epochs of inconceivable remoteness in the past.'

#### *Descent Not a Corollary of Succession.*

Note that Huxley spoke of succession, not of descent. Succession undoubtedly was recognised, but the relation between the terms of the succession was little understood, and there was no proof of descent. Let us suppose all written records to be swept away, and an attempt made to reconstruct English history from coins. We could set out our monarchs in true order, and we might suspect that the throne was hereditary; but if on that assumption we were to make James I. the son of Elizabeth—well, but that's just what palaeontologists are constantly doing. The famous diagram of the Evolution of the Horse which Huxley used in his American lectures has had to be corrected in the light of the fuller evidence recently tabulated in a handsome volume by Professor H. F. Osborn and his coadjutors. *Palaeotherium*, which Huxley regarded as a direct ancestor of the horse, is now held to be only a collateral, as the last of the Tudors were collateral ancestors of the Stuarts. The later *Anchitherium* must be eliminated from the true line as a side-branch—a Young Pretender. Sometimes an apparent succession is due to immigration of a distant relative from some other region—'The glorious House of Hanover and Protestant Succession.' It was, you will remember, by such migrations that Cuvier explained the renewal of life when a previous fauna had become extinct. He admitted succession but not descent. If he rejected special creation, he did not accept evolution.

Descent, then, is not a corollary of succession. Or, to broaden the statement, history is not the same as evolution. History is a succession of events. Evolution means that each event has sprung from the preceding one. Not that the preceding event was the active cause of its successor, but that it was a necessary condition of it. For the evolu-

tionary biologist, a species contains in itself and its environment the possibility of producing its successor. The words 'its environment' are necessary, because a living organism cannot be conceived apart from its environment. They are important, because they exclude from the idea of organic evolution the hypothesis that all subsequent forms were implicit in the primordial protoplast alone, and were manifested either through a series of degradations, as when Thorium by successive disintegrations transmutes itself to Lead, or through fresh developments due to the successive loss of inhibiting factors. I say 'a species contains the possibility' rather than 'the potentiality,' because we cannot start by assuming any kind of innate power.

Huxley, then, forty years ago, claimed that palaeontologists had proved an orderly succession. To-day we claim to have proved evolution by descent. But how do we prove it? The neontologist, for all his experimental breeding, has scarcely demonstrated the transmutation of a species. The palaeontologist cannot assist at even a single birth. The evidence remains circumstantial.

#### *Recapitulation as Proof of Descent.*

Circumstantial evidence is convincing only if inexplicable on any other admissible theory. Such evidence is, I believe, afforded by palaeontological instances of Haeckel's law—i.e., the recapitulation by an individual during its growth of stages attained by adults in the previous history of the race. You all know how this has been applied to the ammonites; but any creatures with a shell or skeleton that grows by successive additions and retains the earlier stages unaltered can be studied by this method. If we take a chronological series of apparently related species or mutations,  $a^1$ ,  $a^2$ ,  $a^3$ ,  $a^4$ , and if in  $a^4$  we find that the growth stage immediately preceding the adult resembles the adult  $a^3$ , and that the next preceding stage resembles  $a^2$ , and so on; if this applies *mutatis mutandis* to the other species of the series; and if, further, the old age of each species foreshadows the adult character of its successor; then we are entitled to infer that the relation between the species is one of descent. Mistakes are liable to occur for various reasons, which we are learning to guard against. For example, the perennial desire of youth to attain a semblance of maturity leads often to the omission of some steps in the orderly process. But this and other eccentricities affect the earlier rather than the later stages, so that it is always possible to identify the immediate ancestor, if it can be found. Here we have to remember that the ancestor may not have lived in the same locality, and that therefore a single cliff-section does not always provide a complete or simple series. An admirable example of the successful search for a father is provided by R. G. Carruthers in his paper on the evolution of *Zaphrentis delanoueï* (1910, *Quart. Journ. Geol. Soc.*, lxi., 523). Surely when we get a clear case of this kind we are entitled to use the word 'proof,' and to say that we have not merely observed the succession, but have proved the filiation.

It has, indeed, been objected to the theory of recapitulation that the stages of individual growth are an inevitable consequence of an

animal's gradual development from the embryo to the adult, and therefore prove nothing. Even now there are those who maintain that the continuity of the germ-plasm is inconsistent with any true recapitulation. Let us try to see what this means. Take any evolutionary series, and consider the germ-plasm at any early stage in it. The germ, it is claimed, contains the factors which produce the adult characters of that stage. Now proceed to the next stage of evolution. The germ has either altered or it has not. If it has not altered, the new adult characters are due to something outside the germ, to factors which may be in the environment but are not in the germ. In this case the animal must be driven by the inherited factors to reproduce the ancestral form; the modifications due to other factors will come in on the top of this, and if they come in gradually and in the later stages of growth, then there will be recapitulation. There does not seem to be any difficulty here. You may deny the term 'character' to these modifications, and you may say that they are not really inherited, that they will disappear entirely if the environment reverts to its original condition. Such language, however, does not alter the fact, and when we pass to subsequent stages of evolution and find the process repeated, and the recapitulation becoming longer, then you will be hard put to it to imagine that the new environment produces first the effects of the old and then its own particular effect.

Even if we do suppose that the successive changes in, say, an ammonite as it passes from youth to age are adaptations to successive environments, this must mean that there is a recapitulation of environment. It is an explanation of structural recapitulation, but the fact remains. There is no difficulty in supposing an individual to pass through the same succession of environments as were encountered in the past history of its race. Every common frog is an instance. The phenomenon is of the same nature as the devious route followed in their migrations by certain birds, a route only to be explained as the repetition of past history. There are, however, many cases, especially among sedentary organisms, which cannot readily be explained in this way.

Let us then examine the other alternative and suppose that every evolutionary change is due to a change in the germ—how produced we need not now inquire. Then, presumably, it is claimed that at each stage of evolution the animal will grow from the egg to the adult along a direct path. For present purposes we ignore purely larval modifications, and admit that the claim appears reasonable. The trouble is that it does not harmonise with facts. The progress from youth to age is not always a simple advance. The creature seems to go out of its way to drag in a growth-stage that is out of the straight road, and can be explained only by the fact that it is inherited from an ancestor. Thus, large ammonites of the *Xipheroceras planicosta* group, beginning smooth, pass through a ribbed stage, which may be omitted, through unituberculate and bituberculate stages, back to ribbed and smooth again. The anal plate of the larval *Antedon*, which ends its course and finally disappears above the limits of the cup, begins life in that lower position which the similar plate occupied in most of the older crinoids.

Here, then, is a difficulty. It can be overcome in two ways. A view held by many is that there are two kinds of characters: first, those that arise from changes in the germ, and appear as sudden or discontinuous variations; second, those that are due to external (*i.e.*, non-germinal) factors. It seems a corollary of this view that the external characters should so affect the germ-plasm as ultimately to produce in it the appropriate factors. This is inheritance of acquired characters. The other way out of the difficulty is to suppose that all characters other than fluctuations or temporary modifications are germinal; that changes are due solely to changes in the constitution of the germ; and that, although a new character may not manifest itself till the creature has reached old age, nevertheless it was inherent in the germ and latent through the earlier growth-stages. This second hypothesis involves two further difficulties. It is not easy to formulate a mechanism by which a change in the constitution of the germ shall produce a character of which no trace can be detected until old age sets in: such a character, for instance, as the tuberculation of the last-formed portion of an ammonite shell. Again, it is generally maintained that characters due to this change of germinal factors, however minute they may be, make a sudden appearance. They are said to be discontinuous. They act as integral units. Now the characters we are trying to explain seem to us palaeontologists to appear very gradually, both in the individual and in the race. Their beginnings are small, scarcely perceptible; they increase gradually in size or strength; and gradually they appear at earlier and earlier stages in the life-cycle. It appears least difficult to suppose that characters of this kind are not initiated in the germ, and that they, if no others, may be subject to recapitulation. It may not yet be possible to visualise the whole process by which such characters are gradually established, or to refer the phenomena of recapitulation back to more fundamental principles. But the phenomena are there, and if any hypothesis is opposed to them so much the worse for the hypothesis. However they be explained, the instances of recapitulation afford convincing proof of descent, and so of genetic evolution.

#### *The 'Line upon Line' Method of Palaeontology*

You will have observed that the precise methods of the modern palaeontologist, on which this proof is based, are very different from the slap-dash conclusions of forty years ago. The discovery of *Archaeopteryx*, for instance, was thought to prove the evolution of Birds from Reptiles. No doubt it rendered that conclusion extremely probable, especially if the major premiss—that evolution *was* the method of nature—were assumed. But the fact of evolution is precisely what men were then trying to prove. These jumpings from Class to Class or from Era to Era, by aid of a few isolated stepping-stones, were what Bacon calls Anticipations, 'hasty and premature' but 'very effective, because as they are collected from a few instances, and mostly from those which are of familiar occurrence, they immediately dazzle the intellect and fill the imagination' (*Nov. Org.* I. 28). No secure step was taken until the modern palaeontologist began to affiliate mutation with mutation and species with species, working his way back, literally

inch by inch, through a single small group of strata. Only thus could he base on the laboriously collected facts a single true Interpretation; and to those who preferred the broad path of generality his Interpretations seemed, as Bacon says they always 'must seem, harsh and discordant—almost like mysteries of faith.'

It is impossible to read these words without thinking of one 'naturae minister et interpres,' whose genius was the first in this country to appreciate and apply to palaeontology the *Novum Organon*. Devoting his whole life to abstruse research, he has persevered with this method in the face of distrust and has produced a series of brilliant studies which, whatever their defects, have illuminated the problems of stratigraphy and gone far to revolutionise systematic palaeontology. Many are the workers of to-day who acknowledge a master in Sydney Savory Buckman.

I have long believed that the only safe mode of advance in palaeontology is that which Bacon counselled and Buckman has practised, namely, 'uniformly and step by step.' Was this not indeed the principle that guided Linnaeus himself? Not till we have linked species into lineages, can we group them into genera; not till we have unravelled the strands by which genus is connected with genus can we draw the limits of families. Not till that has been accomplished can we see how the lines of descent diverge or converge, so as to warrant the establishment of Orders. Thus by degrees we reject the old slippery stepping-stones that so often toppled us into the stream, and foot by foot we build a secure bridge over the waters of ignorance.

The work is slow, for the material is not always to hand, but as we build we learn fresh principles and test our current hypotheses. To some of these I would now direct your attention.

### *Continuity in Development.*

Let us look first at this question of continuity. Does an evolving line change by discontinuous steps (saltations), as when a man mounts a ladder; or does it change continuously, as when a wheel rolls uphill? The mere question of fact is extraordinarily difficult to determine. Considering the gaps in the geological record one would have expected palaeontologists to be the promulgators of the hypothesis of discontinuity. They are its chief opponents.<sup>1</sup> The advocates of discontinuity maintain that palaeontologists are misled: that the steps are so minute as to escape the observation of workers handicapped by the obscurities of their material; that many apparent characters are compound and cannot, in the case of fossils, be subjected to Mendelian

<sup>1</sup> As Dr. W. D. Matthew (1910, *Pop. Sci. Monthly*, p. 473) has well exemplified by the history of the Tertiary oreodont mammals in North America, the known record, taken at its face value leads to 'the conclusion that new species, new genera and even larger groups have appeared by saltatory evolution, not by continuous development.' But a consideration of the general conditions controlling evolution and migration among land mammals shows him that such a conclusion is unwarranted. 'The more complete the series of specimens, the more perfect the record in successive strata, and the nearer the hypothetical centre of dispersal, the closer do we come to a phyletic series whose intergrading stages are well within the limits of observed variation of the race.'

analysis; that no palaeontologist can guarantee the genetic purity of the assemblages with which he works, even when his specimens are collected from a single locality and horizon. It is difficult to reply to such negative arguments. One can but give examples of the kind of observation on which palaeontologists rely.

Since Dr. Rowe's elaborate analysis of the species of *Micraster* occurring in the Chalk of S.E. England, much attention has been concentrated on the gradual changes undergone by those sea-urchins in the course of ages. The changes observed affect many characters; indeed, they affect the whole test, and all parts are doubtless correlated. The changes come in regularly and gradually; there is no sign of discontinuity. It is convenient to give names to the successive forms, but they are linked up by innumerable gradations. There does not seem here to be any question of the sudden appearance of a new character, in one or in many individuals; or of the introduction of any character and the gradual extension of its range by cross-breeding until it has become universal and in turn gives way to some new step in advance. The whole assemblage is affected and moves forward in line, not with an advanced scout here and a straggler there. Slight variation between contemporaneous individuals occurs, no doubt, but the limits are such that a trained collector can tell from a single fossil the level at which it has been found. The continuity of the changes is also inferred from such a fact as that in occasional specimens of *Micraster cor-bovis* the distinctness of the ambulacral sutures (which is one of these characters) is greater on one side of the test than on the other.

Such changes as these may profitably be compared with those which Professor Duerden believes to be taking place in the ostrich. He too finds a slow continuous change affecting innumerable parts of the bird, a change that is universal and within slight limits of variation as between individuals. Even on the hypothesis that every barb of every feather is represented by a factor in the germ, he finds it impossible to regard the changes as other than continuous, and he is driven to the supposition (on the hypothesis of germinal factors) that the factors themselves undergo a gradual change, which he regards as due to old age. It is interesting also that he finds an occasional lop-sided change, such as we noted in *Micraster cor-bovis*.

Whatever may be the explanation, the facts do seem to warrant the statement that evolutionary change can be, and often is, continuous. Professor De Vries has unfortunately robbed palaeontologists of the word 'mutation,' by which, following Waagen, they were accustomed to denote such change. I propose, therefore, to speak of it as 'transition.' But here the question may be posed, whether such transitions can progress indefinitely, or whether they should not be compared to those divergences from the norm of a species which we call fluctuations, because, like the waves of the sea piled up by a gale, they return to their original level when the external cause is removed. If every apparent transition in time is of the latter nature, then, when it reaches a limit comparable to that circumscribing contemporary fluctuation, there must, if progress is to persist, be some disturbance provoking



a saltation, and so giving a new centre to fluctuation and a fresh limit to the upward transition. Those who maintain such a hypothesis presumably regard transition as the response of the growing individual body to gradual change of the physical environment (somatic modification). But saltation they ascribe to a change in the composition of the germ. That change may be forced on the germ by the condition of the body, and may therefore be in harmony with the environment, and may produce a new form along that line. The new form may be obviously distinct from its predecessor, or the range of its fluctuation may overlap that of its predecessor, in which case it will be impossible to decide whether the change is one of transition or of saltation. This succession of hypotheses involves a good many difficulties; among others, the mechanism by which the germ is suddenly modified in accordance with the transition of the body remains obscure. But the facts before us seem to necessitate either perpetual transition, or saltation acting in this manner. Transition, we must admit, also involves a change of the germ *pari passu* with the change of the body. Consequently the difference between the two views seems to be narrowed down to a point which, if not trivial, is at any rate minute.

The particular saltation-hypothesis which I have sketched may remind some hearers of the 'expression points' of E. D. Cope. That really was quite a different conception. Cope believed that, in several cases, generic characters, after persisting for a long time, changed with relative rapidity. This took place when the modifications of adult structure were pushed back so far prior to the period of reproduction as to be transmitted to the offspring. The brief period of time during which this rapid change occurred in any genus was an expression-point, and was compared by Cope to the critical temperature at which a gas changes into a liquid, or a liquid into a solid. The analogy is not much more helpful than Galton's comparison of a fluctuating form to a rocking polyhedron, which one day rocks too much and topples over on to another face. It is, however, useful to note Cope's opinion that these points were 'attained without leaps, and abandoned without abruptness.' He did not believe that 'sports' had 'any considerable influence on the course of evolution' (1887, 'Origin of Fittest,' pp. 39, 79; 1896, 'Factors Org. Evolution,' pp. 24, 25).

#### *The Direction of Change: Seriation.*

The conception of connected change, whether by transition or by scarcely perceptible saltation, or by a combination of the two processes, leads us to consider the Direction of the Change.

Those who attempt to classify species now living frequently find that they may be arranged in a continuous series, in which each species differs from its neighbours by a little less or a little more; they find that the series corresponds with the geographical distribution of the species; and they find sometimes that the change affects particular genera or families or orders, and not similar assemblages subjected, apparently, to the same conditions. They infer from this that the series represents a genetic relation, that each successive species is the descendant of its preceding neighbour; and in some cases this inference

is warranted by the evidence of recapitulation, a fact which further indicates that the change arises by addition or subtraction at the end of the individual life-cycle. So far as I am aware this phenomenon, at least so far as genera are concerned, was first precisely defined by Louis Agassiz in his 'Essay on Classification,' 1857. He called it 'Serial Connection,' a term which connotes the bare statement of fact. Cope in his 'Origin of Genera,' 1869, extended the observation, in a few cases, to species, and introduced the term 'Successional Relation,' which for him implied descent. We may here use the brief and non-committal term 'Seriation.'

The comparison of the seriation of living species and genera to the seriation of a succession of extinct forms as revealed by fossils was, it seems, first definitely made by Cope, who in 1866 held the zoological regions of to-day to be related to one another 'as the different subdivisions of a geologic period in time' (*Journ. Acad. Nat. Sci. Philadelphia*, 1866, p. 108). This comparison is of great importance. Had we the seriations of living forms alone, we might often be in doubt as to the meaning of the phenomenon. In the first place we might ascribe it purely to climatic and similar environmental influence, and we should be unable to prove genetic filiation between the species. Even if descent were assumed we should not know which end of the series was ancestral, or even whether the starting point might not be near the middle. But when the palaeontologist can show the same, or even analogous, seriation in a time-succession, he indicates to the neontologist the solution of his problem.

Here it is well to remind ourselves that all seriations are not exact. There are seriations of organs or of isolated characters, and the transition has not always taken place at the same rate. Hence numerous examples of what Cope called Inexact Parallelism. The recognition of such cases is largely responsible for the multiplication of genera by some modern palaeontologists. This may or may not be the best way of expressing the facts, but it is desirable that they should be plainly expressed or we shall be unable to delineate the actual lines of genetic descent.

Restricting ourselves to series in which descent may be considered as proved or highly probable, such as the Micrasters of the Chalk, we find then a definite seriation. There is not merely transition, but transition in orderly sequence such as can be represented by a graphic curve of simple form. If there are gaps in the series as known to us, we can safely predict their discovery; and we can prolong the curve backwards or forwards, so as to reveal the nature of ancestors or descendants.

#### *Orthogenesis: Determinate Variation.*

The regular, straightforward character of such seriation led Eimer to coin the term Orthogenesis for the phenomenon as a whole. If this term be taken as purely descriptive, it serves well enough to denote certain facts. But Orthogenesis, in the minds of most people, connotes the idea of necessity, of determinate variation, and of predetermined course. Now, just as you may have succession without evolution, so you may have seriation without determination or predetermination.

Let us be clear as to the meaning of these terms. Variation is said to be determinate or, as Darwin called it, 'definite,' when all the offspring vary in the same direction. Such definite variation may be determined by a change in the composition of the germ, due perhaps to some external influence acting on all the parents; or it may express the direct action of an external influence on the growing offspring. The essential feature is that all the changes are of the same kind, though they may differ in degree. For instance, all may consist in some addition, as a thickening of skeletal structures, an outgrowth of spines or horns; or all may consist in some loss, as the smaller size of outer digits, the diminution of tubercles, or the disappearance of feathers. A succession of such determinate variations for several generations produces seriation; and when the seriation is in a plus direction it is called progressive (anabatic, anagenetic), when in a minus direction, retrogressive (catabatic, catagenetic). When successive additions appear late in the life-cycle, each one as it were pushing its predecessors back to earlier stages, then we use Cope's phrase—acceleration of development. When subtraction occurs in the same way, there is retardation of development. Now it is clear that if a single individual or generation produces offspring with, say, plus variations differing in degree, then the new generation will display seriation. Instances of this are well known. You may draw from them what inferences you please, but you cannot actually prove that there is progression. Breeding-experiments under natural conditions for a long series of years would be required for such proof. Here, again, the palaeontologist can point to the records of the process throughout centuries or millennia, and can show that there has been undoubted progression and retrogression. I do not mean to assert that the examples of progressive and retrogressive series found among fossils are necessarily due to the seriation of determinate variations; but the instances of determinate variation known among the creatures now living show the palaeontologist a method that may have helped to produce his series. Once more the observations of neontologist and palaeontologist are mutually complementary.

### *Predetermination.*

So much for determination: now for Predetermination. This is a far more difficult problem, discussed when the fallen angels

'reasoned high  
Of providence, foreknowledge, will, and fate,  
Fixed fate, free will, foreknowledge absolute,  
And found no end in wandering mazes lost.'

—and it is likely to be discussed so long as a reasoning mind persists. For all that, it is a problem on which many palaeontologists seem to have made up their minds. They agree (perhaps unwittingly) with Aristotle\* that 'Nature produces those things which, being con-

\* φύσει γὰρ [γίνονται] ὅσα ἀπὸ τίνος ἐν αὐτοῖς ἀρχῇς συνεχῶς κινούμενα ἀφικνεῖται εἰς τι τέλος. *Physics*, II., 199b, 15, ed. Bekker.

tinuously moved by a certain principle inherent in themselves, arrive at a certain end.' In other words, a race once started on a certain course, will persist in that course; no matter how conditions may change, no matter how hurtful to the individual its own changes may be, progressive or retrogressive, up hill and down hill, straight as a Roman road, it will go on to that appointed end. Nor is it only palaeontologists who think thus. Professor Duerden has recently written, 'The Nagelian idea that evolutionary changes have taken place as a result of some internal vitalistic force, acting altogether independently of external influences, and proceeding along definite lines, irrespective of adaptive considerations, seems to be gaining ground at the present time among biologists' (1919, *Journ. Genetics* viii. p. 193).

The idea is a taking one, but is it really warranted by the facts at our disposal? We have seen, I repeat, that succession does not imply evolution, and (granting evolution) I have claimed that seriation can occur without determinate variation and without predetermination. It is easy to see this in the case of inanimate objects subjected to a controlling force. The fossil-collector who passes his material through a series of sieves, picking out first the larger shells, then the smaller, and finally the microscopic foraminifera, induces a seriation in size by an action which may be compared to the selective action of successive environments. There is, in this case, predetermination imposed by an external mind, but there is no determinate variation. You may see in the museum at Leicester a series beginning with the *via strata* of the Roman occupants of Britain, and passing through all stages of the tramway up to the engineered modern railroad. The unity and apparent inevitability of the series conjures up the vision of a world-mind consciously working to a foreseen end. An occasional experiment along some other line has not been enough to obscure the general trend; indeed, the speedy scrapping of such failures only emphasises the idea of a determined plan. But closer consideration shows that the course of the development was guided simply by the laws of mechanics and economics, and by the history of discovery in other branches of science. That alone was the nature of the determination; and predetermination, there was none. From these instances we see that selection can, indeed must, produce just that evolution along definite lines which is the supposed feature of orthogenesis.

The arguments for orthogenesis are reduced to two: first, the difficulty of accounting for the incipient stages of new structures before they achieve selective value; second, the supposed cases of non-adaptive or even—as one may term it—counter-adaptive growth.

The earliest discernible stage of an entirely new character in an adaptive direction is called by H. F. Osborn a 'rectigradation' (1907), and the term implies that the character will proceed to develop in a definite direction. As compared with changes of proportion in existing characters ('allometron,' Osborn), rectigradations are rare. Osborn applies the term to the first signs of folding of the enamel in the teeth of the horse. Another of his favourite instances is the genesis of horns in the *Titanotheres*, which he has summarised as follows: '(a) from excessively rudimentary beginnings, *i.e.* rectigradations, which can

hardly be detected on the surface of the skull; (b) there is some pre-determining law or similarity of potential which governs their first existence, because (c) the rudiments arise independently on the same part of the skull in different phyla [*i.e.* lineages] at different periods of geologic time; (d) the horn rudiments evolve continuously, and they gradually change in form (*i.e.* allometrons); (e) they finally become the dominating characters of the skull, showing marked variations of the form in the two sexes; (f) they first appear in late or adult stages of ontogeny, but are pushed forward gradually into earlier and earlier ontogenetic stages until they appear to arise prenatally.'

Osborn says that rectigradations are a result of the principle of determination, but this does not seem necessary. In the first place, the precise distinction between an allometron and a rectigradation fades away on closer scrutiny. When the rudiment of a cusp or a horn changes its form, the change is an allometron; the first swelling is a rectigradation. But both of these are changes in the form of a pre-existing structure; there is no fundamental difference between a bone with an equable curve and one with a slight irregularity of surface. Why may not the original modification be due to the same cause as the succeeding ones? The development of a horn in mammalia is probably a response to some rubbing or butting action which produces changes first in the hair and epidermis. One requires stronger evidence than has yet been adduced to suppose that in this case form precedes function. As Jaekel has insisted, skeletal formation follows the changes in the softer tissues as they respond to strains and stresses. In the evolution of the Echinoid skeleton, any new structures that appear, such as auricles for the attachment of jaw-muscles or notches for the reception of external gills, have at their inception all the character of rectigradations, but it can scarcely be doubted that they followed the growth of their correlated soft parts, and that these latter were already subject to natural selection. But we may go further: in vertebrates as in echinoderms the bony substance is interpenetrated with living matter, which renders it directly responsive to every mechanical force, and modifies it as required by deposition or resorption, so that the skeleton tends continually to a correlation of all its parts and an adaptation to outer needs.

The fact that similar structures are developed in the same positions in different stocks at different periods of time is paralleled in probably all classes of animals; Ammonites, Brachiopods, Polyzoa, Crinoids, Sea-urchins present familiar instances. But do we want to make any mystery of it? The words 'predisposition,' 'predetermining law,' 'similarity of potential,' 'inhibited potentiality,' and 'periodicity,' all tend to obscure the simple statement that like causes acting on like material produce like effects. When other causes operate, the result is different. Certainly such facts afford no evidence of predetermination, in the sense that the development must take place willy-nilly. Quite the contrary: they suggest that it takes place only under the influence of the necessary causes. Nor do they warrant such false analogies as 'Environment presses the button: the animal does the rest.'

The resemblance of the cuttle-fish eye to that of a vertebrate has

been explained by the assumption that both creatures are descended, *longo intervallo* no doubt, from a common stock, and that the flesh or the germ of that stock had the internal impulse to produce this kind of eye some day when conditions should be favourable. It is not explained why many other eyed animals, which must also have descended from this remote stock, have developed eyes of a different kind. Nevertheless I commend this hypothesis of Professor Bergson to the advocates of predisposition. To my mind it only shows that a philosopher may achieve distinction by a theory of evolution without a secure knowledge of biology.

When the same stock follows two quite different paths to the same goal, it is impossible to speak of a predetermined course. In the Wenlock beds is a crinoid whose stalk has become flattened and coiled, and the cirri or tendrils of the stalk are no longer set by fives all round it, but are reduced to two rows, one along each side. In one species these cirri are spaced at irregular intervals along the two sides, but as the animal grows there is a tendency for them to become more closely set. In another species, in various respects more developed, the cirri are set quite close together, and the tightly coiled stalk looks like a ribbed ammonite. Closer inspection shows that this species includes two distinct forms. In one each segment of the stalk bears but a single cirrus, first on the right, then on the left; but the segments taper off to the opposite side so that the cirri are brought close together. In the other form two cirri are borne by a single segment, but the next segment bears no cirri. These intervening segments taper to each side, so that here also the cirri are brought close together. Thus the same appearance and the same physiological effect are produced in two distinct ways. Had one of these never existed, the evolution of this curious stem would have offered as good an argument for orthogenesis as many that have been advanced. So much for similarity!

The argument for orthogenesis based on a race-history that marches to inevitable destruction, heedless of environmental factors, has always seemed to me incontrovertible, and so long as my knowledge of palaeontology was derived mainly from books I accepted this premiss as well founded. Greater familiarity with particular groups has led me to doubt whether such cases really occur, for more intensive study generally shows that characters at first regarded as indifferent or detrimental may have been adapted to some factor in the environment or some peculiar mode of life.

Professor Duerden's interesting and valuable studies of the ostrich (1919, 1920, *Journ. Genetics*) lead him to the opinion that retrogressive changes in that bird are destined to continue, and 'we may look forward,' he says, 'to the sad spectacle of a wingless, legless, and featherless ostrich if extinction does not supervene.' Were this so we might at least console ourselves with the thought that the process is a very slow one, for Dr. Andrews tells me that the feet and other known bones of a Pliocene ostrich are scarcely distinguishable from those of the present species. But, after careful examination of Dr. Duerden's arguments, I see no ground for supposing that the changes are other than adaptive. Similar changes occur in other birds of other stocks

when subjected to the requisite conditions, as the flightless birds of diverse origin found on ocean islands, the flightless and running rails, geese, and other races of New Zealand, the Pleistocene *Genyornis* of the dried Lake Callabonna, which, as desert conditions came on, began to show a reduction of the inner toe. Among mammals the legs and feet have been modified in the same way in at least three distinct orders or suborders, during different periods, and in widely separated regions. Living marsupials in Australia have the feet modified according to their mode of life, whether climbing on trees or running over hard ground; and among the latter we find a series indicating how the outer toes were gradually lost and the fourth digit enlarged. I need scarcely remind you of the modifications that resulted in the horse's hoof with its enlarged third digit, traceable during the Tertiary Epoch throughout the Northern Hemisphere, whether in one or more than one stock. I would, however, recall the fact that occasional races, resuming from time to time a forest habitat, ceased to progress along the main line. Lastly, there are those early hoofed animals from South America, made known by Ameghino under the name *Litopterna*, which underwent a parallel series of changes and attained in *Thoatherium* from the Upper Miocene of Patagonia a one-toed foot with elongate metacarpals essentially similar to that of the horse. In all these cases the correlation of foot-structure with mode of life (as also indicated by the teeth) is such that adaptation by selection has always been regarded as the sole effective cause.

My colleague, Dr. W. D. Lang, has recently published a most thoughtful paper on this subject (1919, *Proc. Geol. Assoc.* xxx. 102). His profound studies on certain lineages of Cretaceous Polyzoa (Cheilostomata) have led him to believe that the habit of secreting calcium carbonate, when once adopted, persists in an increasing degree. Thus in lineage after lineage the habit 'has led to a brilliant but comparatively brief career of skeleton-building, and has doomed the organism finally to evolve but the architecture of its tomb.' 'These creatures, like all others which secrete calcium carbonate, are simply suffering from a gouty diathesis, to which each race will eventually succumb. Meanwhile the organism does its best to dispose of the secretion; if usefully, so much the better; but at any rate where it will be least in the way. Some primitive polyzoa, we are told, often sealed themselves up; others escaped this self-immurement by turning the excess into spines, which in yet other forms fused into a front wall. But the most successful architects were overwhelmed at last by superabundance of building-material.'

While sympathetic to Dr. Lang's diagnosis of the disease (for in 1888 I hazarded the view that in Cephalopoda lime-deposition was uncontrollable by the animal, and that its extent was inversely relative to the rate of formation of chitin or other calcifiable tissue), still I think he goes too far in postulating an 'insistent tendency.' He speaks of living matter as if it were the over-pumped inner-tube of a bicycle tyre, 'tense with potentiality, curbed by inhibitions' [of the cover] and 'periodically breaking out as inhibitions are removed' [by broken glass]. A race acquires the lime habit or the drink habit,

and, casting off all restraint, rushes with accelerated velocity down the easy slope to perdition.

A melancholy picture! But is it true? The facts in the case of the Cretaceous Polyzoa are not disputed, but they can be interpreted as a reaction of the organism to the continued abundance of lime-salts in the sea-water. If a race became choked off with lime, this perhaps was because it could not keep pace with its environment. Instead of 'irresistible momentum' from within, we may speak of irresistible pressure from without. Dr Lang has told us (1919, *Phil. Trans. B. ccix.*) 'that in their evolution the individual characters in a lineage are largely independent of one another.' It is this independence, manifested in differing trends and differing rates of change, that originates genera and species. Did the evolution follow some inner impulse, along lines 'predetermined and limited by innate causes,' one would expect greater similarity, if not identity, of pattern and of tempo.

Many are the races which, seeking only ornament, have (say our historians) perished like Tarpeia beneath the weight of a less welcome gift: oysters, ammonites, hippurites, crinoids, and corals. But I see no reason to suppose that these creatures were ill-adapted to their environment—until the situation changed. This is but a special case of increase in size. In creatures of the land probably, and in creatures of the water certainly (as exemplified by A. D. Mead's experiments on the starfish, 1900, *Amer. Natural.* xxxiv. 17), size depends on the amount of food, including all body- and skeleton-building constituents. When food is plentiful larger animals have an advantage over small. Thus by natural selection the race increases in size until a balance is reached. Then a fall in the food-supply handicaps the larger creatures, which may become extinct. So simple an explanation renders it quite unnecessary to regard size as in itself indicating the old age of the race.

Among the structures that have been most frequently assigned to some blind growth-force are spines or horns, and when they assume a grotesque form or disproportionate size they are dismissed as evidences of senility. Let us take a case.

The Trilobite family Lichadidae is represented in Ordovician and Silurian rocks by species with no or few spines, but in the early part of the Devonian, both in America and in Europe, various unrelated groups in this family begin to grow similarly formed and situated spines, at first short and straight, but soon becoming long curved horns, until the climax is reached in such a troll-like goat-form as *Ceratarges armatus* of the Calceola-beds in the Eifel.

Dr. J. M. Clarke (1913, *Monogr. Serv. Geol. Brazil*, i. p. 142) is among those who have regarded this parallel development as a sign of orthogenesis in the most mystical meaning of the term. Strange though these little monsters may be, I cannot, in view of their considerable abundance, believe that their specialisation was of no use to them, and I am prepared to accept the following interpretation by Dr. Rudolf Richter (1919, 1920).

Such spines have their first origin in the tubercles which form so



common an ornament in Crustacea and other Arthropods and which serve to stiffen the carapace. A very slight projection of any of these tubercles further acts as a protection against such soft-bodied enemies as jelly-fish. Longer out-growths enlarge the body of the trilobite in such a way as to prevent its being easily swallowed. When, as is often the case, the spines stretch over such organs as the eyes, their protective function is obvious. This becomes still more clear when we consider the relation of these spines to the body when rolled up, for then they are seen to form an encircling or enveloping *chevaux-de-frise*. But besides this, the spines in many cases serve as balancers; they throw the centre of gravity back from the weighty head, and thus enable the creature to rise into a swimming posture. Further, by their friction, they help to keep the animal suspended in still water with a comparatively slight motion of its numerous oar-like limbs. Regarded in this light, even the most extravagant spines lose their mystery and appear as consequences of natural selection. A comparison of the curious *Marrella* in the pelagic or still-water fauna of the Middle Cambrian Burgess shale with *Acidaspis radiata* of the Calceola-beds certainly suggests that both of these forms were adapted to a similar life in a similar environment.

The fact that many extreme developments are followed by the extinction of the race is due to the difficulty that any specialised organism or machine finds in adapting itself to new conditions. A highly specialised creature is one adapted to quite peculiar circumstances; very slight external change may put it out of harmony, especially if the change be sudden. It is not necessary to imagine any decline of vital force or exhaustion of potentiality.

When people talk of certain creatures, living or extinct, as obviously unadapted for the struggle of life, I am reminded of Sir Henry de la Beche's drawing of a lecture on the human skull by Professor Ichthyosaurus. 'You will at once perceive,' said the lecturer, 'that the skull before us belonged to one of the lower orders of animals; the teeth are very insignificant, the power of the jaws trifling; and altogether it seems wonderful how the creature could have procured food.'

What, then, is the meaning of 'momentum' in evolution? Simply this, that change, whatever its cause, must be a change of something that already exists. The changes in evolving lineages are, as a rule, orderly and continuous (to avoid argument the term may for the moment include minute saltations). Environment changes slowly and the response of the organism always lags behind it, taking small heed of ephemeral variations.<sup>2</sup> Suppose a change from shallow to deep water

<sup>2</sup> The conception of lag in evolution is of some importance. On a hypothesis of selection from fortuitous variations the lag must be considerable. If the variations be determinate and in the direction of the environmental change, the lag will be reduced; but according as the determination departs from the environmental change, the lag will increase. If a change of environment acts on the germ, inducing either greater variation or variation in harmony with the change, there will still be lag, but it will be less. On this hypothesis the lag will depend on the mechanism through which the environment affects the germ. If, with Osborn, we imagine an action on the body, transmitted to the various

—either by sinking of the sea-floor or by migration of the organism. Creatures already capable of becoming acclimatised will be the majority of survivors, and among them those which change most rapidly will soon dominate. Place your successive forms in order, and you will get the appearance of momentum; but the reality is inertia yielding with more or less rapidity to an outer force.

Sometimes a change is exhibited to a greater or less extent by every member of some limited group of animals, and this change may seem to be correlated with the conditions of life in only a few of the genera or species, while in others it manifests no adaptive character and no selective value. Thus the loss of the toes or even limbs in certain lizards is ascribed by Dr. G. A. Boulenger to an internal tendency, although, at any rate in the Skinks, which furnish examples of all stages of loss, it certainly seems connected with a sand-loving and burrowing life. Recently Dr. Boulenger (1920, *Bull. Soc. Zool. France*, xlv. 68) has put forward the East African *Testudo loveridgei*, a ribless tortoise with soft shell that squeezes into holes under rocks, and swells again like an egg in a bottle, as the final stage of a regressive series. The early stages of this regression, such as a decrease in size of the vertebral processes and rib-heads, were long since noticed by him in other members of the same family; but, since they did not occur in other families, and since he could perceive no adaptive value in them, he regarded them as inexplicable, until this latest discovery proved them to be prophetic of a predestined goal. The slightness of my acquaintance with tortoises forbids me to controvert this supreme example of teleology as it appears to so distinguished an authority. But in all these apparent instances we should do well to realise that we are still incompletely informed about the daily life of these creatures and of their ancestors in all stages of growth, and we may remember that structures once adaptive often persist after the need has passed or has been replaced by one acting in a different direction.

### *The Study of Adaptive Form.*

This leads us on to consider a fruitful field of research, which would at first seem the natural preserve of neontologists, but which, as it happens, has of late been cultivated mainly to supply the needs of palaeontology. That field is the influence of the mode of life on the shape of the creature, or briefly, of function on form; and, conversely, the indications that form can give as to habits and habitat. For many a long year the relatively simple mechanics of the vertebrate skeleton have been studied by palaeontologists and anatomists generally, and have been brought into discussions on the effect of use. The investigation of the mechanical conditions controlling the growth of organisms has recently been raised to a higher plane by Professor D'Arcy Thompson's

parts through catalyzers and hormones, then the process will involve lag varying with the physico-chemical constitution of the organism. Slight differences in this respect between different races may have some bearing on the rate of change (*vide infra* 'The Tempo of Evolution'), on the correlation of characters, and so on the diversity of form.

suggestive book on 'Growth and Form.' These studies, however, have usually considered the structure of an animal as an isolated machine. We have to realise that an organism should be studied in relation to the whole of its environment, and here form comes in as distinct from structure. That mode of expression, though loose and purely relative, will be generally understood. By 'form' one means those adaptations to the surrounding medium, to food, to the mode of motion, and so forth, which may vary with outer conditions while the fundamental structure persists. Though all structures may, conceivably, have originated as such adaptations, those which we call 'form' are, as a rule, of later origin. Similar adaptive forms are found in organisms of diverse structure, and produce those similarities which we know as 'convergence.' To take but one simple instance from the relations of organisms to gravity. A stalked echinoderm naturally grows upright, like a flower, with radiate symmetry. But in the late Ordovician and in Silurian rocks are many in which the body has a curiously flattened leaf-like shape, in which the two faces are distinct, but the two sides alike, and in which this effect is often enhanced by paired outgrowths corresponding in shape if not in structure. Expansion of this kind implies a position parallel to the earth's surface, *i.e.* at right angles to gravity. The leaf-like form and the balancers are adaptations to this unusual position. Recognition of this enables us to interpret the peculiar features of each genus, to separate the adaptive form from the modified structure, and to perceive that many genera outwardly similar are really of quite different origin.

Until we understand the principles governing these and other adaptations—irrespective of the systematic position of the creatures in which they appear—we cannot make adequate reconstructions of our fossils, we cannot draw correct inferences as to their mode of life, and we cannot distinguish the adaptive from the fundamental characters. No doubt many of us, whether palaeontologists or neontologists, have long recognised the truth in a general way, and have attempted to describe our material—whether in stone or in alcohol—as living creatures; and not as isolated specimens but as integral portions of a mobile world. It is, however, chiefly to Louis Dollo that we owe the suggestion and the example of approaching animals primarily from the side of the environment, and of studying adaptations as such. The analysis of adaptations in those cases where the stimulus can be recognised and correlated with its reaction (as in progression through different media or over different surfaces) affords sure ground for inferences concerning similar forms of whose life-conditions we are ignorant. Thus Othenio Abel (1916) has analysed the evidence as to the living squids and cuttle-fish and has applied it to the belemnites and allied fossils with novel and interesting results. But from such analyses there have been drawn wider conclusions pointing to further extension of the study. It was soon seen that adaptations did not come to perfection all at once, but that harmonisation was gradual, and that some species had progressed further than others. But it by no means follows that these represent chains of descent. The adaptations of all the organs must be considered, and one seriation checked by another. Thus in 1890, in sketching the probable

history of certain crinoids, I pointed out that the seriation due to the migration of the anal plates must be checked by the seriation due to the elaboration of arm-structure, and so on.

In applying these principles we are greatly helped by Dollo's thesis of the Irreversibility of Evolution. It is not necessary to regard this as an absolute Law, subject to no conceivable exception. It is a simple statement of the facts as hitherto observed, and may be expressed thus:

1. In the course of race-history an organism never returns exactly to its former state, even if placed in conditions of existence identical with those through which it has previously passed. Thus, if through adaptation to a new mode of life (as from walking to climbing) a race loses organs which were highly useful to it in the former state, then, if it ever reverts to that former mode of life (as from climbing to walking), those organs never return, but other organs are modified to take their place.

2. But (continues the Law), by virtue of the indestructibility of the past, the organism always preserves some trace of the intermediate stages. Thus, when a race reverts to its former state, there remain the traces of those modifications which its organs underwent while it was pursuing another mode of existence.

The first statement imposes a veto on any speculations as to descent that involve the reappearance of a vanished structure. It does not interfere with the cases in which old age seems to repeat the characters of youth, as in Ammonites, for here the old-age character may be similar, but obviously is not the same. The second statement furnishes a guide to the mode of life of the immediate ancestors, and is applicable to living as well as to fossil forms. It is from such persistent adaptive characters that some have inferred the arboreal nature of our own ancestors, or even of the ancestors of all mammals. To take but a single point, Dr. W. D. Matthew (1904, *Amer. Natural.* xxxviii. 813) finds traces of a former opposable thumb in several early Eocene mammals, and features dependent on this in the same digit of all mammals where it is now fixed.

### *The Study of Habitat.*

The natural history of marine invertebrata is of particular interest to the geologist, but its study presents peculiar difficulties. The marine zoologist has long recognised that his early efforts with trawl and dredge threw little light on the depth in the sea frequented by his captures. The surface floaters, the swimmers of the middle and lower depths, and the crawlers on the bottom were confused in a single haul, and he has therefore devised means for exploring each region separately. The geologist, however, finds all these faunas mixed in a single deposit. He may even find with them the winged creatures of the air, as in the insect beds of Gurnet Bay, or the remains of estuarine and land animals.

Such mixtures are generally found in rocks that seem to have been deposited in quiet land-locked bays. Thus in a Silurian rock near Visby, Gotland, have been found creatures of such diverse habitat as a scorpion, a possibly estuarine *Pterygotus*, a large barnacle, and a crinoid of the delicate form usually associated with clear deep water.

The lagoons of Solenhofen have preserved a strange mixture of land and sea life, without a trace of fresh or brackish water forms. *Archaeopteryx*, insects, flying reptiles, and creeping reptiles represent the air and land fauna; jelly-fish and the crinoid *Succocoma* are true open-water wanderers; sponges and stalked crinoids were sessile on the bottom; starfish, sea-urchins, and worms crawled on the sea-floor; king-crabs, lobsters, and worms left their tracks on mud-flats; cephalopods swam at various depths; fishes ranged from the bottom mud to the surface waters. The Upper Ordovician Starfish bed of Girvan contains not only the crawling and wriggling creatures from which it takes its name, but stalked echinoderms adapted to most varied modes of life, swimming and creeping trilobites, and indeed representatives of almost all marine levels.

In the study of such assemblages we have to distinguish between the places of birth, of life, of death, and of burial, since, though these may all be the same, they may also be different. The echinoderms of the Starfish bed further suggest that closer discrimination is needed between the diverse habitats of bottom forms. Some of these were, I believe, attached to sea-weed; others grew up on stalks above the bottom; others clung to shells or stones; others lay on the top of the sea-floor; others were partly buried beneath its muddy sand; others may have grovelled beneath it, connected with the overlying water by passages. Here we shall be greatly helped by the investigations of C. G. J. Petersen and his fellow-workers of the Danish Biological Station. (See especially his summary, 'The Sea Bottom and its Production of Fish Food,' Copenhagen, 1918.) They have set an example of intensive study which needs to be followed elsewhere. By bringing up slabs of the actual bottom, they have shown that, even in a small area, many diverse habitats, each with its peculiar fauna, may be found, one superimposed on the other. Thanks to Petersen and similar investigators, exact comparison can now take the place of ingenious speculation. And that this research is not merely fascinating in itself, but illuminatory of wider questions, follows from the consideration that analysis of faunas and their modes of life must be a necessary preliminary to the study of migrations and geographical distribution.

### *The Tempo of Evolution.*

We have not yet done with the results that may flow from an analysis of adaptations. Among the many facts which, when considered from the side of animal structure alone, lead to transcendental theories with Greek names, there is the observation that the relative rate of evolution is very different in races living at the same time. Since their remains are found often side by side, it is assumed that they were subject to the same conditions, and that the differences of speed must be due to a difference of internal motive force. After what has just been said you will at once detect the fallacy in this assumption. Professor Abel has recently maintained that the varying tempo of evolution depends on the changes in outer conditions. He compares the evolution of whales, sirenians, and horses during the Tertiary Epoch, and correlates it with

the nature of the food. Roughly to summarise, he points out that from the Eocene onwards the sirenians underwent a steady, slow change, because, though they migrated from land to sea, they retained their habit of feeding on the soft water-plants. The horses, though they remained on land, display an evolution at first rather quick, then slower, but down to Pliocene times always quicker than that of the sirenians; and this is correlated with their change into eaters of grain, and their adaptation to the plains which furnish such food. The whales, like the sirenians, migrated at the beginning of the Tertiary from land to sea; but how different is their rate of evolution, and into what diverse forms have they diverged! At first they remained near the coasts, keeping to the ancestral diet, and, like the sirenians, changing but slowly. But the whales were flesh-eaters, and soon they took to hunting fish, and then to eating large and small cephalopods; hence from the Oligocene onwards the change was very quick, and in Miocene times the evolution was almost tempestuous. Finally, many whales turned to the swallowing of minute floating organisms, and from Lower Pliocene times, having apparently exhausted the possibilities of ocean provender, they changed with remarkable slowness.

Whether such changes of food or of other habits of life are, in a sense, spontaneous, or whether they are forced on the creatures by changes of climate and other conditions, makes no difference to the facts that the changes of form are a reaction to the stimuli of the outer world, and that the rate of evolution depends on those outer changes.

Whether we have to deal with similar changes of form taking place at different times or in different places, or with diverse changes affecting the same or similar stocks at the same time and place, we can see the possibility that all are adaptations to a changing environment. There is then reason for thinking that ignorance alone leads us to assume some inexplicable force urging the races this way or that, to so-called advance or to apparent degeneration, to life or to death.

#### *The Rhythm of Life.*

The comparison of the life of a lineage to that of an individual is, up to a point, true and illuminating; but when a lineage first starts on its independent course (which really means that some individuals of a pre-existing stock enter a new field), then I see no reason to predict that it will necessarily pass through periods of youth, maturity, and old age, that it will increase to an acme of numbers, of variety, or of specialisation, and then decline through a second childhood to ultimate extinction. Still less can we say that, as the individuals of a species have their allotted span of time, long or short, so the species or the lineage has its predestined term. The exceptions to those assertions are indeed recognised by the supporters of such views, and they are explained in terms of rejuvenescence, rhythmic cycles, or a grand despairing outburst before death. This phraseology is delightful as metaphor, and the conceptions have had their value in promoting search for confirmatory or contradictory evidence. But do they lead to any broad and fructifying principle? When one analyses them one is perpetually brought up against some transcendental assumption, some

unknown entelechy that starts and controls the machine, but must for ever evade the methods of our science.

The facts of recurrence, of rhythm, of rise and fall, of marvellous efflorescences, of gradual decline, or of sudden disappearances, all are incontestable. But if we accept the intimate relation of organism and environment, we shall surmise that on a planet with such a geological history as ours, with its recurrence of similar physical changes, the phenomena of life must reflect the great rhythmic waves that have uplifted the mountains and lowered the deeps, no less than every smaller wave and ripple that has from age to age diversified and enlivened the face of our restless mother.

To correlate the succession of living forms with all these changes is the task of the palaeontologist. To attempt it he will need the aid of every kind of biologist, every kind of geologist. But this attempt is not in its nature impossible, and each advance to the ultimate goal will, in the future as in the past, provide both geologist and biologist with new light on their particular problems. When the correlation shall have been completed, our geological systems and epochs will no longer be defined by gaps in our knowledge, but will be the true expression of the actual rhythm of evolution. Lyell's great postulate of the uniform action of nature is still our guide; but we have ceased to confound uniformity with monotony. We return, though with a difference, to the conceptions of Cuvier, to those numerous and relatively sudden revolutions of the surface of the globe which have produced the corresponding dynasties in its succession of inhabitants.

#### *The Future.*

The work of a systematic palaeontologist, especially of one dealing with rare and obscure fossils, often seems remote from the thought and practice of modern science. I have tried to show that it is not really so. But still it may appear to some to have no contact with the urgent problems of the world outside. That also is an error. Whether the views I have criticised or those I have supported are the correct ones is a matter of practical importance. If we are to accept the principle of predetermination, or of blind growth-force, we must accept also a check on our efforts to improve breeds, including those of man, by any other means than crossings and elimination of unfit strains. In spite of all that we may do in this way, there remain those decadent races, whether of ostriches or human beings, which 'await alike the inevitable hour.' If, on the other hand, we adopt the view that the life-history of races is a response to their environment, then it follows, no doubt, that the past history of living creatures will have been determined by conditions outside their control, it follows that the idea of human progress as a biological law ceases to be tenable; but, since man has the power of altering his environment and of adapting racial characters through conscious selection, it also follows that progress will not of necessity be followed by decadence; rather that, by aiming at a high mark, by deepening our knowledge of ourselves and of our world, and by controlling our energy and guiding our efforts in the light of that knowledge, we may prolong and hasten our ascent to ages and to heights as yet beyond prophetic vision.

# British Association for the Advancement of Science.

SECTION D: CARDIFF, 1920.

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## ADDRESS TO THE ZOOLOGICAL SECTION

BY

PROFESSOR J. STANLEY GARDINER, M.A., F.R.S.,

PRESIDENT OF THE SECTION.

*Where do we stand?*

THE public has the right to consider and pass judgment on all that affects its civilisation and advancement, and both of these largely depend on the position and advance of science. I ask its consideration of the science of Zoology, whether or not it justifies its existence as such, and, if it does, what are its needs. It is at the parting of the ways. It either has to justify itself as a science or be altogether starved out by the new-found enthusiasm for chemistry and physics, due to the belief in their immediate application to industries.

It is a truism to point out that the recent developments in chemistry and physics depend, in the main, on the researches of men whose names are scarcely known to the public: this is equally true for all sciences. A list of past Presidents of the Royal Society conveys nothing to the public compared with a list of Captains of Industry who, to do them justice, are the first to recognise that they owe their position and wealth to these scientists. These men of science are unknown to the public, not on account of the smallness of their discoveries, but rather on account of their magnitude, which makes them meaningless to the mass.

Great as have been the results in physical sciences applied to industry, the study of animal life can claim discoveries just as great. Their greatest value, however, lies not in the production of wealth, but rather in their broad applicability to human life. Man is an animal and he is subject to the same laws as other animals. He learns by the experience of his forebears, but he learns, also, by the consideration of other animals in relationship to their fellows and to the world at large. The whole idea of evolution, for instance, is of indescribable value; it permeates all life to-day; and yet Charles Darwin, whose researches did more than any others to establish its facts, is too often only known to the public as 'the man who said we came from monkeys.'



Whilst first and foremost I would base my claim for the study of animal life on this consideration, we cannot neglect the help it has given to the physical welfare of man's body. It is not out of place to draw attention to the manner in which pure zoological science has worked hand in hand with the science of medicine. Harvey's experimental discovery of the circulation of the blood laid the foundation for that real knowledge of the working of the human body which is at the basis of medicine; our experience of the history of its development gives us good grounds to hope that the work that is now being carried out by numerous researchers under the term 'experimental' will ultimately elevate the art of diagnosis into an exact science. Harvey's work, too, mostly on developing chicks, was the starting-point for our knowledge of human development and growth. Instances in medicine could be multiplied wherein clinical treatment has only been rendered possible by laborious research into the life histories of certain parasites preying often on man and other animals alternately. In this connection there seems reason at present for the belief that the great problem of medical science, cancer, will reach its solution from the zoological side. A pure zoologist has shown that typical cancer of the stomach of the rat can be produced by a parasitic threadworm (allied to that found in pork, *Trichina*), thus having as a carrying host the American cockroach, brought over to the large warehouses of Copenhagen in sacks of sugar. Our attack on such parasites is only made effective by what we know of them in lower forms, which we can deal with at will. Millions of the best of our race owe their lives to the labours of forgotten men of science, who laid the foundations of our knowledge of the generations of insects and flat-worms, the modes of life of lice and ticks, and the physiology of such lowly creatures as *Amoeba* and *Paramecium*; parasitic disease—malaria, Bilhaziasis, typhus, trench fever and dysentery—was as deadly a foe to us as was the Hun.

Of immense economic importance in the whole domain of domestic animals and plants was the rediscovery, early in the present century, of the completely forgotten work of Gregor Mendel on cross-breeding, made known to the present generation largely by the labours of a former President of this Association, who, true man of science, claims no credit for himself. We see results already in the few years that have elapsed in special breeds of wheat, in which have been combined with exactitude the qualities man desires. The results are in the making—and this is true of all things in biology—but can anyone doubt that the breeding of animals is becoming an exact science? We have got far, perhaps, but we want to get much further in our understanding of the laws governing human heredity; we have to establish immunity to disease. Without the purely scientific study of chromosomes (the bodies which carry the physical and mental characteristics of parents to children) we could have got nowhere, and to reach our goal we must know more of the various forces which in combination make up what we term life.

In agricultural sciences we are confronted with pests in half a dozen different groups of animals. We have often to discover which of two or more is the damaging form, and the difficulty is greater

where the damage is due to association between plant and animal pests. Insects are, perhaps, the worst offenders, and our basal knowledge of them as living organisms—they can do no damage when dead, and perhaps pinned in our showcases—is due to Redi, Schwammerdam, and Réaumur in the middle of the seventeenth century. Our present successful honey production is founded on the curiosity of these men in respect to the origin of life and the generations of insects. The fact that most of the dominant insects have a worm (caterpillar or maggot) stage of growth, often of far longer duration than that of the insects, has made systematic descriptive work on the relation of worm and insect of peculiar importance. I hesitate, however, to refer to catalogues in which perhaps a million different forms of adults and young are described. Nowadays we know, to a large degree, with what pests we deal and we are seeking remedies. We fumigate and we spray, spending millions of money, but the next remedy is in the use of free-living enemies or parasites to prey on the insect pests. The close correlation of anatomy with function is of use here in that life histories, whether parasitic, carnivorous, vegetarian, or saprophagous, can be foretold in fly maggots from the structure of the front part of their gut (pharynx); we know whether any maggot is a pest, is harmless, or is beneficial.

I won't disappoint those who expect me to refer more deeply to science in respect to fisheries, but its operations in this field are less known to the public at large. The opening up of our north-western grounds and banks is due to the scientific curiosity of Wyville Thomson and his *confrères* as to the existence or non-existence of animal life in the deep sea. It was sheer desire for knowledge that attracted a host of inquirers to investigate the life history of river eels. The wonder of a fish living in our shallowest pools and travelling two or three thousand miles to breed, very likely on the bottom in 2000 fathoms, and subjected to pressures varying from 14 lb. to 2 tons per square inch, is peculiarly attractive. It shows its results in regular eel farming, the catching and transplantation of the baby eels out of the Severn into suitable waters, which cannot, by the efforts of Nature alone, be sure of their regular supply. Purely scientific observations on the life histories of flat fish—these were largely stimulated by the scientific curiosity induced by the views of Lamarck and Darwin as to the causes underlying their anatomical development—and on the feeding value and nature of Thisted Bredning and the Dogger Bank, led to the successful experiments on transplantation of young plaice to these grounds and the phenomenal growth results obtained, particularly on the latter. Who can doubt that this 'movement of herds' is one of the first results to be applied in the farming of the North Sea as soon as the conservation of our fish supply becomes a question of necessity?

The abundance of mackerel is connected with the movements of Atlantic water into the British Channel and the North Sea, movements depending on complex astronomical, chemical, and physical conditions. They are further related to the food of the mackerel, smaller animal life which dwells only in these Atlantic waters. These depend, as indeed do all animals, on that living matter which possesses chlorophyll

for its nutrition and which we call plant. In this case the plants are spores of algae, diatoms, etc., and their abundance as food again depends on the amount of the light of the sun—the ultimate source, it might seem, of all life.

A method of ascertaining the age of fishes was sought purely to correlate age with growth in comparison with the growth of air-living vertebrates. This method was found in the rings of growth in the scales, and now the ascertaining of age-groups in herring shoals enables the Norwegian fishermen to know with certainty what possibilities and probabilities are before them in the forthcoming season. From the work on the blending together of Atlantic with Baltic and North Sea water off the Baltic Bight and of the subsequent movements of this Bank water, as it is termed, into the Swedish fiords can be understood, year by year, the Swedish herring fishery. It is interesting that these fisheries have been further correlated with cycles of sun spots, and also with longer cycles of lunar changes.

The mass of seemingly unproductive scientific inquiries undertaken by the United States Bureau of Fisheries, thirty to fifty years ago, was the forerunner of their immense fish-hatching operations, whereby billions of fish eggs are stripped year by year and the fresh waters of that country made into an important source for the supply of food. The study of the growth stages of lobsters and crabs has resulted in sane regulations to protect the egg-carrying females, and in some keeping up of the supply in spite of the enormously increased demand. Lastly, the study of free-swimming larval stages in mollusca, stimulated immensely by their similarity to larval stages in worms and starfishes, has given rise to the establishment of a successful pearl-shell farm at Dongonab, in the Red Sea, and of numerous fresh-water mussel fisheries in the southern rivers of the United States, to supply small shirt buttons.

Fishery investigation was not originally directed to a more ambitious end than giving a reasonable answer to a question of the wisdom or unwisdom of compulsorily restricting commercial fishing, but it was soon found that this answer could not be obtained without the aid of pure zoology. The spread of trawling—and particularly the introduction of steam trawling during the last century—gave rise to grave fears that the stock of fish in home waters might be very seriously depleted by the use of new methods. We first required to know the life histories of the various trawled fish, and Sars and others told us that the eggs of the vast majority of the European marine food species were pelagic; in other words, that they floated, and thus could not be destroyed, as had been alleged. Trawl fishing might have to be regulated all the same, for there might be an insufficient number of parents to keep up the stock. It was clearly necessary to know the habits, movements, and distribution of the fishes, for all were not, throughout their life, or at all seasons, found on the grounds it was practicable to fish. A North Sea plaice of 12 in. in length, a quite moderate size, is usually five years old. The fact that of the female plaice captured in the White Sea, a virgin ground, the vast majority are mature, while less than half the plaice put upon our

markets from certain parts of the southern North Sea in the years immediately before the war had ever spawned, is not only of great interest, but gives rise to grave fears as to the possibility of unrestricted fishing dangerously depleting the stock itself. There is, however, another group of ideas surrounding the question of getting the maximum amount of plaice-meat from the sea; it may be that the best size for catching is in reality below the smallest spawning size. I here merely emphasise that in the plaice we have an instance of an important food fish, whose capture it will probably be necessary to regulate, and that in determining how best the stock may be conserved, what sizes should receive partial protection, on what grounds fish congregate and why, and in all the many cognate questions which arise, answers to either can only be given by the aid of zoological science.

But why multiply instances of the applications of zoology as a pure science to human affairs? Great results are asked for on every side of human activities. The zoologist, if he be given a chance to live and to hand on his knowledge and experience to a generation of pupils, can answer many of them. He is increasingly getting done with the collection of anatomical facts, and he is turning more and more to the why and how animals live. We may not know in our generation nor in many generations what life is, but we can know enough to control that life. The consideration of the fact that living matter and water are universally associated opens up high possibilities. The experimental reproduction of animals, without the interposition of the male, is immensely interesting; where it will lead no one can foretell. The association of growth with the acidity and alkalinity of the water is a matter of immediate practical importance, especially to fisheries. The probability of dissolved food material in sea and river water, independent of organised organic life and absorbable over the whole surfaces of animals, is clearly before us. Is it possible that that dissolved material may be even now being created in nature without the assistance of organic life? The knowledge of the existence in food of vitamins, making digestible and usable what in food would otherwise be wasted, may well result in economies of food that will for generations prevent the necessity for the artificial restriction of populations. The parallel between these vitamins and something in sea-water may quite soon apply practically to the consideration of all life in the sea. Finally, what we know of the living matter of germ cells puts before us the not impossible hope that we may influence for the better the generations yet to come.

If it is the possibility in the unknown that makes a science, are there not enough possibilities here? Does Zoology, with these problems before it, look like a decayed and worked-out science? Is it not worthy to be ranked with any other science, and is it not worthy of the highest support? Is it likely to show good value for the money spent upon it? Should we not demand for it a Professorial Chair in every University that wishes to be regarded as an educational institution? And has not the occupant of such a Chair a task at least equal in difficulty to that of the occupant of any other Chair? Surely the zoologist may reasonably claim an equal position and pay to that

of the devotee of any other science! The researcher is not a huckster and will not make this claim on his own behalf, but the occupant of this Chair may be allowed to do so for him.

So far I have devoted my attention primarily, in this survey of the position of Zoology, to the usefulness of the subject. Let us now note where we stand in respect to other subjects and in meeting the real need for wide zoological study.

All sciences are now being reviewed, and zoology has to meet month by month the increasingly powerful claim of physics and chemistry for public support. Both of these sciences are conspicuously applicable to industry, and this, perhaps, is their best claim. The consideration of life as a science would itself be in danger were it not for the economic applications of physiology to medicine. This is the danger from without, but there is another from within, and this lies in the splitting up of the subject into a series of small sections devoted to special economic ends. They are a real danger in that they are forming enclosures within a science, while research is more and more breaking down the walls between sciences. Zoology in many Universities scarcely exists, for what is assimilated by agriculturalists and medical men are catalogued lists of pests, while medical students merely acquire the technique of observing dead forms of animals other than human—not the intention of the teachers, it is true, but a melancholy fact all the same. The student, I say again, is merely acquiring in 'Zoology' a travesty of a noble subject, but to this point I return later.

Let me now give a few facts which have their sweet and bitter for us who make Zoology our life work. During the war we wanted men who had passed the Honours Schools in Zoology—and hence, were presumably capable of doing the work—to train for the diagnosis of protozoal disease. We asked for all names from 1905 to 1914 inclusive, and the average worked out at under fourteen per year from all English Universities: an average of one student per University per year. In the year 1913-14 every student who had done his Honours Course in Zoology in 1913 could, if he had taken entomology as his subject, have been absorbed into the economic applications of that subject. Trained men were wanted to undertake scientific fishery investigations and they could not be found. Posts were advertised in Animal Breeding, in Helminthology, and in Protozoology, three other economic sides of the subject. The Natural History Museum wanted systematists and there were many advertisements for teachers. How many of these posts were filled I don't know, but it is clear that not more than one-half—or even one-third—can have been filled efficiently. Can any zoologist say that all is well with his subject in the face of these deficiencies?

The demands for men in the economic sides of zoology are continually growing, and it is the business of Universities to try and meet these demands. There are Departments of Government at home and in our Colonies, which, in the interests of the people they govern, wish to put into operation protective measures but cannot do so because there are not the men with the requisite knowledge and common sense required for Inspectorates. There are others that wish for research

to develop seas to conserve existing industries as well as to discover new ones, and they, too, are compelled to mark time.

In default, or in spite of, the efforts of the schools of pure zoology, attempts are being made to set up special training schools in fisheries, in entomology, and in other economic applications of zoology. Each branch is regarded as a science and the supporters of each suppose they can, from the commencement of a lad's scientific training, give specialised instruction in each. The researcher in each has to do the research which the economic side requires. But he can't restrict his education to one science; he requires to know the principles of all sciences; he must attempt to understand what life is. Moreover, his specialist knowledge can seldom be in one science. The economic entomologist, however deep his knowledge of insects may be, will find himself frequently at fault in distinguishing cause and effect unless he has some knowledge of mycology. The protozoologist must have an intimate knowledge of unicellular plants, bacterial and other. The animal-breeder must know the work on cross-fertilisation of plants. The fisheries man requires to understand physical oceanography. The helminthologist and the veterinary surgeon require an intimate knowledge of a rather specialised 'physiology.' All need knowledge of the comparative physiology of animals in other groups beyond those with which they deal, to assist them in their deductions and to aid them to secure the widest outlook. It is surely a mistake, while the greatest scientific minds of the day find that they require the widest knowledge, to endeavour to get great scientific results out of students whose training has been narrow and specialised. Such specialisation requires to come later, and can replace nothing. This short cut is the longest way round. The danger is not only in our science, but in every science.

In face of this highly gratifying need for trained zoologists, independently of medical schools, I ask my colleagues in the teaching of zoology, 'What is wrong with our schools of zoology that they are producing so few men of science? It is not the subject! Can it be our presentation of it, or is it merely a question of inadequate stipends?'

In science schools there can be no standing still. Progress or retrogression in thought, technique, and method are the two alternatives. If we are to progress we must see ever wider vistas of thought, and must use the achievements of our predecessors as the take-off for our own advances. The foundations of our science were well and truly laid, but we must not count the bricks for ever, but add to them. Far be it from me to decry the knowledge and ideas our predecessors have given to us. To have proved the possibility, nay, probability, that all life is one life and that life itself is permanent is an immense achievement. To have catalogued the multitudinous forms that life takes in each country was a herculean task. To have studied with meticulous care the shapes, forms, and developments of organs in so many bodies was equally herculean. It was as much as could be expected in the nineteenth century, during most of which zoology was in advance of all other sciences. But surely for these pioneer workers this docketing, tabulating, and collecting was not the object of their research, but the means to its attainment. The prize they sought was the under-

standing of life itself, the intangible mystery which makes ourselves akin to all these specimens, the common possession which gives to man, as to the lowest creature, the power of growth and reproduction.

To my colleagues I say, let us no longer, in the reconstruction immediately before us, tie ourselves down to the re-chewing of our dry bones. They are but dead bones, and the great mystery which once lived in them has passed from them, and it is that we must now seek. Not in bones, in myriads of named specimens, does that mystery dwell, but in the living being itself, in the growth and reproduction of live creatures. Observation and experiment rather than tabulation and docketing are our technique. What is that life, common to you, to me, to our domestic pets, to animals and to plants alike? Surely this is our goal, and the contents of our museums, means to this end, are in danger of being regarded as the end. There is hope now. Those of us who have the will to look can see zoology in its proper place, the colleague of botany in applying physics and chemistry to the understanding of life itself. The study of life is the oldest of all sciences; it is the science in which the child earliest takes an interest; its study has all the attributes required for education of the highest type, for the appreciation of the beauty of form and of music, of unselfishness, of self-control, of imagination, of love, and constancy. The more we know of life, the more we appreciate its wonders and the more we want to know; it is good to be alive.

Surely the time has now come for us to lift our eyes from our tables of groups and families, and, on the foundations of the knowledge of these, work on the processes going on in the living body, the adaptation to environment, the problems of heredity, and of many another fascinating hunt in unknown country. Let us teach our students not only what is known, but, still more, what is unknown, for in the pursuit of the latter we shall engage eager spirits who care nought for collections of corpses. My own conviction is that we are in danger of burying our live subject along with our specimens in museums.

We see the same evil at work in the teaching of zoology from the very beginning. Those of us who are parents know that the problems of life assail a child almost as soon as it can speak, and that it is the animal side of creation which makes the most natural and immediate appeal to its interest and curiosity. Where such interest is intelligent and constant it is safe to educate truly in the desired direction. You will notice that the child's questions are very fundamental and that, according to my experience, the facts elicited are applied widely, and with perfect simplicity. Thus my own small daughter, having elicited where the baby rabbits came from, said 'Oh! just like eggs from hens.'

The child's own desires show up best what his mind requires for its due development, and I fear no contradiction in claiming that it is animal life in all its living aspects. Yet what is he given? Schools encourage 'natural history,' as it is termed. In some it is nature; but too often it consists in a series of prizes for dates—when the first blooms of wild flowers were found; the first nests, eggs, and young of birds; the records of butterflies and moths, etc. Actual instruction, if there is any beyond this systematic teaching of destruction, frequently

lies solely in a few sheets of the life histories of the cabbage butterfly and other insects. Fossil sea urchins and shells are curiosities and are used to teach names. The whole is taught—there are some striking exceptions—with the minimum requirements of observation and intelligence. Plants too often dominate. The lad can pluck flowers and tear up roots; there is a certain cruelty to be discouraged if animals are treated similarly, but here there is none, for 'they are not alive' as we are. Which one of us would agree to this, and say that there is not a similar 'cruelty' in tearing up plants? The method is the negation of science. The boy must be taught from the other end, from the one animal about which he does know a little, viz., himself. From the commencement he must associate himself with all living matter. The child—boy or girl—shows us the way in that he is invariably keener on the domestic pets, while he has to be bribed by pennies to learn plant names.

As a result of the wrong teaching of zoology we see proposals to make so-called 'nature study' in our schools purely botanical. Is this proposal made in the interests of the teacher or the children? It surely can't be for 'decency' if the teaching is honest, for the phenomena are the same, and there is nothing 'indecent' common to all life. 'The proper study of mankind is man,' and the poor child, athirst for information about himself, is given a piece of moss or duckweed, or even a chaste buttercup. Is the child supposed to get some knowledge it can apply economically? Whatever the underlying ideas may be, this course will not best develop the mind to enable it to grapple with all phenomena, the aim of education. If necessary, the school teacher must go to school; he must bring himself up to date in his own time, as every teacher in science has to do; it is the business of Universities to help him, for nothing is more important to all science than the foundations of knowledge.

Into schools is now moving the teaching required for the first professional examination in medicine, and this profoundly affects the whole attitude of teachers. It has a syllabus approved by the Union of Medicine, the 'apprenticeship' to which is as real and as difficult to alter as that of any expert trade with its own union. It compels the remembering of a number of anatomical facts relating to a miscellaneous selection of animals and plants, and the acquirement of a certain amount of technique. However it may be taught, its examination can almost invariably be passed on memory and manual dexterity; it implies no standard of mental ability. Anatomy without function and knowledge of an organism without reference to its life is surely futile. And yet, too often, this is what our colleagues concerned with the second year of this apprenticeship directly or indirectly compel us to teach in the first year. Surely it is time for us to rebel and insist that what is required is education as to the real meaning of what life is. We shall never reach complete agreement as to a syllabus, but probably we are all at one in regarding reproduction as the most interesting biological phenomenon, and water and air as the most important environments.

Unfortunately most Universities have adopted this in many ways



unscientific and rather useless first Medical Examination as part of their first examination for the B.Sc. degree and for diplomas and degrees in agriculture, dentistry, and other subjects. Zoology is part of a syllabus in which half a dozen professors are concerned, and it cannot change with the times without great difficulty. Our colleagues of other sciences do not want it to change, preferring that a rival subject to attract pupils should remain in a backwash; to be just, each has firm belief in the subject he knows. For our continuation courses, having choked out the more thinking students, we have to go on as we have begun, and we survey the animal kingdom in a more or less systematic manner. We carefully see that all our beasts are killed before we commence upon them; we deal solely with their comparative anatomy, to which are often added some stories of 'evolution,' the whole an attempted history of the animal kingdom. There are great educational merits in the study of the comparative anatomy of a group of similar animals, but too often we go to group after group, the student attaining all that is educational in the first, only securing from each subsequent group more and more facts which might just as well be culled from text-books.

Students who continue further and take the final honours in zoology specialise in most Universities in their last year in some branch of their science. Such students are usually thinking of the subject from the point of view of their subsequent livelihood. They have to think of what will pay and in what branches there is, in their University, some teacher from whom they can get special instruction. They read up the most modern text-book, examine a few specimens, and are often given the class they desire by examiners who know less of their speciality than they do. They are then supposed to be qualified both to teach and research in zoology. They teach on the same vicious lines, and in research many are satisfied to become mere accumulators of more facts in regard to dead creatures.

I have called this address 'Where do we stand?' and I hope all who are interested will try to answer this question. Personally I feel that we stand in a most uncomfortable position, in which, to use a colloquialism, we must either get on or get out. I am certain that the progress of humanity requires us to 'get on.'

Of you in my audience who are not workers in science I ask a final moment of consideration. There is no knowledge of which it is possible to answer the question, 'What is the use of it?' for only time can disclose what are the full bearings of any piece of knowledge. Let us not starve pure research because we do not see its immediate application. I often think that if Sir Isaac Newton, at the present day, discovered the law of gravity as a result of watching the apples fall, someone would say, 'Oh! interesting, no doubt; but my money will go to the man who can stop the maggots in them.'

On the one side leads the path of economic research, offering more obvious attractions in the way of rapid results and of greater immediate recognition. Their path is one trodden by noble steps, full of sacrifice and difficulty, worthy of treading. But let us view with still greater sympathy and understanding the harder path which leads workers,

through years of seemingly unproductive toil, to strive after the solution of the great basal problems of life. Such workers forfeit for themselves the hope of wealth, leisure, and public recognition. As a rule they die in harness, and leave not much beyond honoured names. These are they who worship at the Altar of the Unknown, who at great cost wrest from the darkness its secrets, not recking of the boon they may bring to humanity. It is for these I plead, not for themselves as individuals, but for the means wherewith to keep the flame of pure research burning, for the laboratories and equipment that all Universities need.



# British Association for the Advancement of Science.

SECTION E : CARDIFF, 1920.

## ADDRESS TO THE GEOGRAPHICAL SECTION

BY

JOHN McFARLANE, M.A.,

PRESIDENT OF THE SECTION.

SINCE the last meeting of the British Association, Treaties of Peace have been signed with Austria, Hungary, Bulgaria, and Turkey; and, although there is still much which is unsettled, especially in the East, it is now possible to obtain some idea of the changes wrought on the map of Europe by the Great War. These changes are indeed of the most profound and far-reaching description. Old States have in some cases either disappeared or suffered an enormous loss of territory, and new States, with the very names of which we are but vaguely familiar, have been brought into existence. It has seemed to me, therefore, that it might not be altogether inappropriate to inquire into the principles upon which these territorial changes have been made, and to consider how far the political units affected by them possess the elements of stability. A learned but pessimistic historian to whom I confided my intention shook his head and gravely remarked, 'Whatever you say on that subject will be writ in water.' But the more I consider the matter the more do I feel convinced that certain features in the reconstructed Europe are of great and even of permanent value, and it is in that belief that I have ventured to disregard the warning which was given me.

In the rearrangement of European States which has taken place, geographical conditions have perhaps not always had the consideration which they deserve, but in an inquiry such as that upon which we are engaged they naturally occupy the first place. And by geographical conditions I am not thinking primarily, or even mainly, of defensive frontiers. It may be true, as Sir Thomas Holdich implies, that they alone form the true limits of a State. But if they do we ought to carry our theory to its logical conclusion and crown them with barbed-wire entanglements. Whether mankind would be happier or even safer if placed in a series of gigantic compounds I greatly doubt. It is to the land within the frontier, and not to the frontier itself, that our main consideration should be given. The factors which we have to take

into account are those which enable a people to lead a common national life, to develop the economic resources of the region within which they dwell, to communicate freely with other peoples, and to provide not only for the needs of the moment, but as far as possible for those arising out of the natural increase of the population.

The principle of self-determination has likewise played an important, if not always a well-defined, part in the rearrangement of Europe. The basis upon which the new nationalities have been constituted is on the whole ethnical, though it is true that within the main ethnical divisions advantage has been taken of the further differentiation in racial characteristics arising out of differences in geographical environment, history, language, and religion. But no more striking illustration could be adduced of the strength of ethnic relationships at the present time than the union of the Czechs with the Slovaks, or of the Serbs with the Croats and the Slovenes. Economic considerations, of course, played a great part in the settlement arrived at with Germany, but on the whole less weight has been attached to them than to ethnic conditions. In cases where they have been allowed to influence the final decision the result arrived at has not always been a happy one. Nor can more be said for those cases where the motive was political or strategic. Historical claims, which have been urged mainly by Powers anxious to obtain more than that to which they are entitled on other grounds, may be regarded as the weakest of all claims to the possession of new territory.

When we come to examine the application of the principles which I have indicated to the settlement of Europe we shall, I think, find that the promise of stability is greatest in those cases where geographical and ethnical conditions are most in harmony, and least where undue weight has been given to conditions which are neither geographical nor ethnical.

The restoration of Alsace-Lorraine to France has always been treated as a foregone conclusion in the event of a successful termination of the war against Germany. From the geographical point of view, however, there are certainly objections to the inclusion of Alsace within French territory. The true frontier of France in that region is the Vosges, not necessarily because they form the best defensive frontier, but because Alsace belongs to the Rhineland, and the possession of it brings France into a position from which trouble with Germany may arise in the future.

Nor can French claims to Alsace be justified on ethnical grounds. The population of the region contains a strong Teutonic element, as indeed does that of Northern France, and the language spoken by over 90 per cent. of the people is German. On the other hand, it must be borne in mind that during the period between the annexation of Alsace by France in the seventeenth century and its annexation by Germany in the nineteenth French policy appears to have been highly successful in winning over the sympathies of the Alsations, just as between 1871 and 1914 German policy was no less successful in alienating them. The restoration of Alsace must therefore be defended, if at all, on the ground that its inhabitants are more attached to France than to Germany. That attachment it will be necessary for France to

preserve in the future, as economic conditions are not altogether favourable. The cotton industry of Alsace may perhaps attach itself to that of France without great difficulty; but the agricultural produce of the Rhine plain will as before be likely to find its best and most convenient market in the industrial regions of Germany.

With regard to Lorraine the position is somewhat different. Physically that region belongs in the main to the country of the Paris basin, and is therefore in a sense part of France. Strategically it commands the routes which enter France from Germany between Belgium and the Vosges, and from that point of view its possession is of the utmost importance to her. Of the native population about one-third speak French, and the German element is mainly concentrated in the more densely populated districts of the north-east. But although in these various aspects Lorraine may be regarded as belonging to France in a sense in which Alsace does not, the real argument for the restoration of the ceded provinces is in both cases the same. Lorraine, no less than Alsace, is French in its civilisation and in its sympathies.

From the economic point of view, however, the great deposits of iron ore in Lorraine constitute its chief attraction for France to-day, just as they appear to have constituted its chief attraction for Germany half a century ago. But the transfer of the province from Germany, which has built up a great industry on the exploitation of its mines, to France, which does not possess in sufficient abundance coal for smelting purposes, together with other arrangements of a territorial or quasi-territorial nature made partly at least in consequence of this transfer, at once raises questions as to the extent to which the economic stability of Germany is threatened. The position of that country with regard to the manufacture of iron and steel will be greatly affected, for not only does she lose, in Lorraine and the Saar, regions in which these manufactures were highly developed, but she loses in them the sources from which other manufacturing regions still left to her, notably the Ruhr, drew considerable quantities either of raw materials or of semi-manufactured goods. For example, in 1913 the Ruhr, which produced over 40 per cent. of the pig iron of the German Empire, obtained 15 per cent. of its iron ore from Lorraine, and it also obtained from there and from the Saar a large amount of pig iron for the manufacture of steel. Unless, therefore, arrangements can be made for a continued supply of these materials a number of its industrial establishments will have to be closed down.

In regard to coal, the position is also serious. We need not, perhaps, be unduly impressed by the somewhat alarmist attitude of Mr. Keynes, who estimates that on the basis of the 1913 figures Germany, as she is now constituted, will require for the pre-war efficiency of her railways and industries an annual output of 110,000,000 tons, and that instead she will have in future only 100,000,000 tons, of which 40,000,000 will be mortgaged to the Allies. In arriving at these figures Mr. Keynes has made an allowance of 18,000,000 tons for decreased production, one-half of which is caused by the German miner having shortened his shift from eight and a half to seven hours per day. This is certainly a deduction which we need not take into account. Mr.

Keynes also leaves out of his calculation the fact that previous to the war about 10,000,000 tons per year were sent from Upper Silesia to other parts of Germany, and there is no reason to suppose that this amount need be greatly reduced, especially in view of Article 90 of the Treaty of Versailles, which provides that 'for a period of fifteen years Poland will permit the produce of the mines of Upper Silesia to be available for sale to purchasers in Germany on terms as favourable as are applicable to like products sold under similar conditions in Poland or in any other country.' We have further to take into account the opportunities for economy in the use of coal, the reduction in the amount which will be required for bunkers, the possibility of renewing imports from abroad—to a very limited extent indeed, but still to some extent—and the fact that the French mines are being restored more rapidly than at one time appeared possible. (On the basis of the production of the first four months of 1920 Germany could already reduce her Treaty obligations to France by 1,000,000 tons per year.) Taking all of these facts into account, it is probably correct to say that when Germany can restore the output of the mines left to her to the 1913 figure, she will, as regards her coal supply for industrial purposes, be in a position not very far removed from that in which she was in 1910, when her total consumption, apart from that at the mines, was about 100,000,000 tons.

The actual arrangements which have been made, it is true, are in some cases open to objection. The Saar is not geographically part of France, and its inhabitants are German by race, language, and sympathy. It is only in the economic necessities of the situation that a defence, though hardly a justification, of the annexation of the coal-field can be found. The coal from it is to be used in the main for the same purposes as before, whereas if it had been left to Germany much of it might have been diverted to other purposes. In 1913 the total production of Alsace-Lorraine and the Saar amounted to about 18,000,000 tons, while their consumption was about 14,000,000 tons. There is thus apparently a net gain to France of about 4,000,000 tons, but from that must be deducted the amount which the North-East of France received from this field in pre-war days. Switzerland also will probably in future continue to draw part of its supplies from the Saar.

The stipulation that Germany should for ten years pay part of her indemnities to France, Belgium, and Italy in kind also indicates an attempt to preserve the pre-war distribution of coal in Europe, though in some respects the scales seem to have been rather unfairly weighted against Germany. France, for example, requires a continuance of Westphalian coal for the metallurgical industries of Lorraine and the Saar, while Germany requires a continuance of Lorraine ore if her iron-works on the Ruhr are not to be closed down. There was therefore nothing unreasonable in the German request that she should be secured her supplies of the latter commodity. Indeed, it would have been to the advantage of both countries if a clause similar to Article 90, which I have already quoted, had been inserted in the Treaty. It is true that temporary arrangements have since been made which will ensure to Germany a considerable proportion of her pre-war consumption of

minette ores. But some agreement which enabled the two separate but complementary natural regions of the Saar and the Ruhr to exchange their surplus products on a business basis would have tended to an earlier restoration of good feeling between the two countries.

One other question which arises in this connection is the extent to which the steel industry of Germany will suffer by the loss of the regions from which she obtained the semi-manufactured products necessary for it. On this subject it is dangerous to prophesy, but when we take into consideration the length of time required for the construction of modern steelworks, the technical skill involved in their management, and the uncertainties with regard to future supplies of fuel, it seems unlikely that France will attempt any rapid development of her steel industry. In that case the Ruhr will still continue to be an important market for Lorraine and the Saar.

Our general conclusion, then, is that the territorial arrangements which have been made do not necessarily imperil the economic stability of Germany. The economic consequences of the war are really much more serious than the economic consequences of the peace. Germany has for ten years to make good the difference between the actual and the pre-war production of the French mines which she destroyed. Her own miners are working shorter hours, and as a result her own production is reduced, and as British miners are doing the same she is unable to import from this country. For some years these deductions will represent a loss to her of about 40,000,000 tons per annum, and will undoubtedly make her position a serious one. But to give her either the Saar or the Upper Silesian coalfields would be to enable her to pass on to others the debt which she herself has incurred. The reduction of her annual deliveries of coal to France, Belgium, and Italy was, indeed, the best way in which to show mercy to her.

The position of Poland is geographically weak, partly because its surface features are such that the land has no well-marked individuality, and partly because there are on the east and west no natural boundaries to prevent invasion or to restrain the Poles from wandering far beyond the extreme limits of their State. Polish geographers themselves appear to be conscious of this geographical infirmity, as Vidal de la Blache would have termed it, and in an interesting little work Nalkowski has endeavoured to show that the very transitionality of the land between east and west entitles it to be regarded as a geographical entity. But transitionality is rather the negation of geographical individuality than the basis on which it may be established. And indeed no one has pointed out its dangers more clearly than Nalkowski himself. 'The Polish people,' he says, 'living in this transitional country always were, and still are, a prey to a succession of dangers and struggles. They should be ever alert and courageous, remembering that on such a transitional plain, devoid of strategic frontiers, racial boundaries are marked only by the energy and civilisation of the people. If they are strong they advance those frontiers by pushing forward; by weakening and giving way they promote their contraction. So the mainland may thrust out arms into the sea, or, being weak, may be breached and even overwhelmed by the ocean



floods.' If we bear in mind the constant temptation to a people which is strong to advance its political no less than its racial frontiers, and the constant danger to which a weakening people is exposed of finding its political frontier contract even more rapidly than its racial, we shall realise some of the evils to which a State basing its existence on transitionality is exposed.

It is, then, to racial feeling, rather than to geographical environment, that we must look for the basis of the new Polish State, but the intensity with which this feeling is likely to operate varies considerably in different parts of the region which it is proposed to include. In the plébiscite area of Upper Silesia there were, according to the census of 1900, which is believed to represent the facts more accurately than that of 1910, seven Poles to three of other nationalities. In Prussian Poland, apart from the western districts which have not been annexed to Poland and the town and district of Bromberg, the Poles number at least 75 per cent. of the total population, and in the ceded and plébiscite areas of East and West Prussia 52 per cent. Russian Poland, which contains rather more than two-thirds of the entire population of what we may call ethnic Poland, has 9,500,000 Poles and over 3,000,000 Jews, Germans, Lithuanians, and others, while West Galicia is almost solidly Polish. Thus out of a total population of 21,000,000 within the regions mentioned the Poles number 15,500,000, or about 75 per cent.

Bearing these facts in mind, it is possible to consider the potentialities of the new State. The population is sufficiently large and the Polish element within it is sufficiently strong to justify its independence on ethnical grounds. Moreover, the alien elements which it contains are united neither by racial ties nor by contiguity of settlement. In Posen, for example, there is in the part annexed to Poland a definitely Polish population with a number of isolated German settlements, while in Russian Poland the Jews are to be found mainly in the towns. Considered as a whole, Poland is at least as pure racially as the United States.

When we consider the economic resources of Poland we see that they also make for a strong and united State. It is true that in the past the country has failed to develop as an economic unit, but this is a natural result of the partitions and of the different economic systems which have prevailed in different regions. Even now, however, we can trace the growth of two belts of industrial activity which will eventually unite these different regions together. One is situated on the coalfield running from Oppeln in Silesia by Cracow and Lemberg, and is engaged in mining, agriculture, and forestry; while the other extends from Posen by Lodz to Warsaw, and has much agricultural wealth and an important textile industry. Moreover, the conditions, geographical and economic, are favourable to the growth of international trade. If Poland obtains Upper Silesia she will have more coal than she requires, and the Upper Silesian fields will, as in the past, export their surplus produce to the surrounding countries, while the manufacturing districts will continue to find

their best markets in the Russian area to the east. The outlets of the State are good, for not only has it for all practical purposes control of the port of Danzig, but it is able to share in the navigation of the Oder and it has easy access to the south by way of the Moravian Gap.

It seems obvious, therefore, that Poland can best seek compensation for the weakness of her geographical position by developing the natural resources which lie within her ethnic frontiers. By such a policy the different parts of the country will be more closely bound to one another than it is possible to bind them on a basis of racial affinity and national sentiment alone. Moreover, Poland is essentially the land of the Vistula, and whatever is done to improve navigation on that river will similarly tend to have a unifying effect upon the country as a whole. The mention of the Vistula, however, raises one point where geographical and ethnical conditions stand in marked antagonism to one another. The Poles have naturally tried to move downstream to the mouth of the river which gives their country what little geographical individuality it possesses, and the Polish corridor is the expression of that movement. On the other hand, the peoples of East and West Prussia are one and the same. The geographical reasons for giving Poland access to the sea are no doubt stronger than the historical reasons for leaving East Prussia united to the remainder of Germany, but strategically the position of the corridor is as bad as it can be, and the solution arrived at may not be accepted as final.

Lastly, we may consider the case of East Galicia, which the Poles claim not on geographical grounds, because it is in reality part of the Ukraine, and not on ethnical grounds, because the great majority of the inhabitants are Little Russians, but on the ground that they are and have for long been the ruling race in the land. It may also be that they are not uninfluenced by the fact that the region contains considerable stores of mineral oil. But as the claim of the Poles to form an independent State is based on the fact that they form a separate race it is obviously unwise to weaken that claim by annexing a land which counts over 3,000,000 Ruthenes to one-third that number of Poles. Further, the same argument which the Poles use in regard to East Galicia could with no less reason be used by the Germans in Upper Silesia. Mr. Keynes, indeed, suggests that the Allies should declare that in their judgment economic conditions require the inclusion of the coal districts of Upper Silesia in Germany unless the wishes of the inhabitants are decidedly to the contrary. It is not improbable that East Galicia would give a more emphatic vote against Polish rule than Upper Silesia will give for it. If Poland is to ensure her position she must forget the limits of her former empire, turn her back on the Russian plain, with all the temptations which it offers, and resolutely set herself to the development of the basin of the Vistula, where alone she can find the conditions which make for strength and safety.

Czecho-Slovakia is in various ways the most interesting country in the reconstructed Europe. Both geographically and ethnically it is marked by some features of great strength, and by others which are a source of considerable weakness to it. Bohemia by its physical

structure and its strategic position seems designed by Nature to be the home of a strong and homogeneous people. Moravia attaches itself more or less naturally to it, since it belongs in part to the Bohemian massif and is in part a dependency of that massif. Slovakia is Carpathian country, with a strip of the Hungarian plain. Thus, while Bohemia possesses great geographical individuality and Slovakia is at least strategically strong, Czecho-Slovakia as a whole does not possess geographical unity and is in a sense strategically weak, since Moravia, which unites the two upland wings of the State, lies across the great route which leads from the Adriatic to the plains of Northern Europe. The country might easily, therefore, be cut in two as the result of a successful attack, either from the north or from the south. Later I shall endeavour to indicate certain compensations arising out of this diversity of geographical features, but for the moment at least they do not affect our argument.

We have, further, to note that the geographical and ethnical conditions are not altogether concordant. In Bohemia there is in the basin of the Eger in the north-west an almost homogeneous belt of German people, and on the north-eastern and south-western borderlands there are also strips of country in which the Germanic element is in a considerable majority. It is no doubt true, as Mr. Wallis has shown, that the Czechs are increasing in number more rapidly than the Germans, but on ethnical grounds alone there are undoubtedly strong reasons for detaching at least the north-western district from the Czecho-Slovak State. We feel justified in arguing, however, that here at least the governing factors are and must be geographical. To partition a country which seems predestined by its geographical features to be united and independent would give rise to an intolerable sense of injustice. I do not regard the matter either from the strategic or from the economic point of view, though both of these are no doubt important. What I have in mind is the influence which the geographical conditions of a country exercise upon the political ideas of its inhabitants. It is easy to denounce, as Mr. Toynbee does, 'the pernicious doctrine of natural frontiers,' but they will cease to appeal to the human mind only when mountain and river, highland and plain cease to appeal to the human imagination. With good sense on both sides the difficulties in this particular case are not insurmountable. The Germans of the Eger valley, which is known as German Bohemia, have never looked to Germany for leadership nor regarded it as their home, and their main desire has hitherto been to form a separate province in the Austrian Empire. A liberal measure of autonomy might convert them into patriotic citizens, and if they would but condescend to learn the Czech language they might come to play an important part in the government of the country.

In Slovakia also there are racial differences. Within the mountain area the Slovaks form the great majority of the population, but in the valleys, and on the plains of the Danube to which the valleys open out, the Magyar element predominates. Moreover, it is the Magyar element which is racially the stronger, and before which the Slovaks are gradually retiring. Geographical and ethnical conditions therefore

unite in fixing the political frontier between Magyar and Slovak at the meeting place of hill and plain. But on the west such a frontier would have been politically inexpedient because of its length and irregularity, and economically disadvantageous because the river valleys, of which there are about a dozen, would have had no easy means of communication with one another or with the outside world. Hence the frontier was carried south to the Danube, and about 1,000,000 Magyars were included in the total population of 3,500,000. Nor is the prospect of assimilating these Magyars particularly bright. The Germans in Bohemia are cut off from the Fatherland by mountain ranges, and, as we have seen, it does not appear to present any great attraction to them. It is otherwise in Slovakia, where the Magyars of the lowland live in close touch with those of the Alföld, and it may be long ere they forget their connection with them. The danger of transferring territory not on geographical or ethnical, but on economic, grounds could not be more strikingly illustrated.

With regard to economic development, the future of the new State would appear to be well assured. Bohemia and Moravia were the most important industrial areas in the old Austrian Empire, and Slovakia, in addition to much good agricultural land, contains considerable stores of coal and iron. But if Czecho-Slovakia is to be knit together into a political and economic unit, its communications will have to be developed. We have already suggested that the geographical diversity of the country offers certain compensation for its lack of unity, but these cannot be taken advantage of until its different regions are more closely knit together than they are at present. The north of Bohemia finds its natural outlet both by rail and water through German ports. The south-east of Bohemia and Moravia look towards Vienna. In Slovakia the railways, with only one important exception, converge upon Budapest. The people appear to be alive to the necessity of remedying this state of affairs, and no fewer than fifteen new railways have been projected, which, when completed, will unite Bohemia and Moravia more closely to one another and Slovakia. Moreover, it is proposed to develop the waterways of the country by constructing a canal from the Danube at Pressburg to the Oder. From this canal another will branch off at Prerau and run to Pardubitz on the Elbe, below which point that river has still to be canalised. If these improvements are carried out the position of Czecho-Slovakia will, for an inland State, be remarkably strong. It will have through communication by water with the Black Sea, the North Sea, and the Baltic, and some of the most important land routes of the Continent already run through it. On the other hand, its access to the Adriatic is handicapped by the fact that in order to reach that sea its goods will have to pass through the territory of two, if not of three, other States, and however well the doctrine of economic rights of way may sound in theory, there are undoubted drawbacks to it in practice. Even with the best intentions, neighbouring States may fail to afford adequate means of transport through defective organisation, trade disputes, or various other reasons. It is probable, therefore, that the development of internal communications will in the end be to the advantage of the German ports, and

more especially of Hamburg. But the other outlets of the State will certainly tend towards the preservation of its economic independence.

The extent to which Rumania has improved her position as a result of the war is for the present a matter of speculation. On the one hand she has added greatly to the territory which she previously held, and superficially she has rendered it more compact; but on the other she has lost her unity of outlook, and strategically at least weakened her position by the abandonment of the Carpathians as her frontier. Again, whereas before the war she had a fairly homogeneous population—probably from 90 to 95 per cent. of the 7,250,000 people in the country being of Rumanian stock—she has, by the annexation of Transylvania, added an area of 22,000 square miles of territory, in which the Rumanians number less than one and a half out of a total of two and two-third millions. In that part of the Banat which she has obtained there is also a considerable alien element. It is in this combination of geographical division and ethnic intermixture that we may foresee a danger to Rumanian unity. That part of the State which is ethnically least Rumanian is separated from the remainder of the country by a high mountain range, and in its geographical outlook no less than in the racial sympathies of a great number of its inhabitants is turned towards the west, while pre-war Rumania remains pointed towards the south-east. Economically also there is a diversity of interest, and the historical tie is perhaps the most potent factor in binding the two regions together. It is not impossible, therefore, that two autonomous States may eventually be established, more or less closely united according to circumstances.

The position in the Dobruja is also open to criticism. Geographically the region belongs to Bulgaria, and the Danube will always be regarded as their true frontier by the Bulgarian people. Ethnically its composition is very mixed, and whatever it was originally, it certainly was not a Rumanian land. But after the Rumanians had rather unwillingly been compelled to accept it in exchange for Bessarabia, flched from them by the Russians, their numbers increased and their economic development of the region, and more especially of the port of Constanza, undoubtedly gave them some claims to the northern part of it. As so often happens, however, when a country receives part of a natural region beyond its former boundaries, Rumania is fertile in excuses for annexing more of the Dobruja. To the southern part, which she received after the Balkan wars, and in the possession of which she has been confirmed by the peace terms with Bulgaria, she has neither ethnically nor economically any manner of right. The southern Dobruja is a fertile area which, before its annexation, formed the natural hinterland of the ports of Varna and Ruschuk. Her occupation of it will inevitably draw Rumania on to further intervention in Bulgarian affairs.

The arrangements which have been made with regard to the Banat must be considered in relation to the Magyar position in the Hungarian plain. The eastern country of the Banat, Krasso-Szörény, has a population which is in the main Rumanian, and as it belongs to the Carpathian area it is rightly included with Transylvania in Rumanian territory. In the remainder of the Banat, including Arad, the Rumanians form less than one-third of the total population, which also

comprises Magyars, Germans, and Serbs. The Hungarian plain is a great natural region, capable of sub-division no doubt, but still a great natural region, in which the Magyar element is predominant. The natural limit of that plain is the mountain region which surrounds it, and to that limit at least the Magyar political power will constantly press. But Rumania has been permitted to descend from the mountains and Jugo-Slavia to cross the great river which forms her natural boundary, and both have obtained a foothold on the plain where it may be only too easy for them to seek occasion for further advances. And it cannot be urged that the principle of self-determination would have been violated by leaving the Western Banat to the Magyars. No plébiscite was taken, and it is impossible to say how the German element would have given what in the circumstances would have been the determining vote. Finally, as it was necessary to place nearly a million Magyars in Transylvania under Rumanian rule, it might not have been altogether inexpedient to leave some Rumanians on Hungarian soil.

For the extension of Jugo-Slavia beyond the Danube two pleas have been advanced, one ethnical and the other strategic. Neither is really valid. It is true that there is a Serbian area to the north of Belgrade, but the total number of Serbs within the part assigned to Jugo-Slavia probably does not much exceed 300,000. The strategic argument that the land which they occupy is necessary for the defence of the capital is equally inconclusive. From the military point of view it does not easily lend itself to defensive operations, and when we consider the political needs of the country we cannot avoid the conclusion that a much better solution might have been found in the removal of the capital to some more central position. The Danube is certainly a better defensive frontier than the somewhat arbitrary line which the Supreme Council has drawn across the Hungarian plain.

In fact, it is in the treatment of the Hungarian plain that we feel most disposed to criticise the territorial settlements of the Peace Treaties. Geographical principles have been violated by the dismemberment of a region in which the Magyars were in a majority, and in which they were steadily improving their position. Ethnical principles have been violated, both in the north, where a distinctly Magyar region has been added to Slovakia, and in the south where the eastern Banat and Bačka have been divided between the Rumanians and the Jugo-Slavs, who together form a minority of the total population. For the transfer of Arad to Rumania and of the Burgenland to Austria more is to be said, but the position as a whole is one of unstable equilibrium, and can only be maintained by support from without. In this part of Europe at least a League of Nations will not have to seek for its troubles.

When we turn to Austria we are confronted with the great tragedy in the reconstruction of Europe. Of that country it could once be said '*Bella gerant alii, tu felix Austria nube,*' but to-day, when dynastic bonds have been loosened, the constituent parts of the great but heterogeneous empire which she thus built up have each gone its own way. And for that result Austria herself is to blame. She failed to realise that an empire such as hers could only be permanently retained on a

basis of common political and economic interest. Instead of adopting such a policy, however, she exploited rather than developed the subject nationalities, and to-day their economic, no less than their political, independence of her is vital to their existence. Thus it is that the Austrian capital, which occupies a situation unrivalled in Europe, and which before the war numbered over 2,000,000 souls, finds herself with her occupation gone. For the moment Vienna is not necessary either to Austria or to the so-called Succession States, and she will not be necessary to them until she again has definite functions to perform. I do not overlook the fact that Vienna is also an industrial city, and that it, as well as various other towns in Lower Austria, are at present unable to obtain either raw materials for their industries or foodstuffs for their inhabitants. But there are already indications that this state of affairs will shortly be ameliorated by economic treaties with the neighbouring States. And what I am particularly concerned with is not the temporary but the permanent effects of the change which has taken place. The entire political re-orientation of Austria is necessary if she is to emerge successfully from her present trials, and such a re-orientation must be brought about with due regard to geographical and ethnical conditions. The two courses which are open to her lead in opposite directions. On the one hand she may become a member of a Danubian confederation, on the other she may throw in her lot with the German people. The first would really imply an attempt to restore the economic position which she held before the war, but it is questionable whether it is either possible or expedient for her to make such an attempt. A Danubian confederation will inevitably be of slow growth, as it is only under the pressure of economic necessity that it will be joined by the various nationalities of south-eastern Europe. The suggestions made by Mr. Asquith, Mr. Keynes, and others, for a compulsory free-trade union would, if carried into effect, be provocative of the most intense resentment among most, if not all, of the States concerned. But even if a Danubian confederation were established it does not follow that Austria would be able to play a part in it similar to that which she played in the Dual Monarchy. With the construction of new railways and the growth of new commercial centres it is probable that much of the trade with the south-east of Europe which formerly passed through Vienna will in future go to the east of that city. Even now Pressburg, or Bratislava, to give it the name by which it will hence be known, is rapidly developing at the expense alike of Vienna and Budapest. Finally, Austria has in the past shown little capacity to understand the Slav peoples, and in any case her position in what would primarily be a Slav confederation would be an invidious one. For these reasons we turn to the suggestion that Austria should enter the German Empire, which, both on geographical and on ethnical grounds, would appear to be her proper place. Geographically she is German, because the bulk of the territory left to her belongs either to the Alpine range or to the Alpine foreland. It is only when we reach the basin of Vienna that we leave the mid-world mountain system and look towards the south-east of Europe across the great Hungarian plain. Ethnically, of course, she is essentially German. Now although my argument hitherto has rather endeavoured

to show that the transfer of territory from one State to another on purely economic grounds is seldom to be justified, it is equally indefensible to argue that two States which are geographically and ethnically related are not to be allowed to unite their fortunes because it would be to their interest to do so. And that it would be to their interest there seems little doubt. Austria would still be able to derive some of her raw materials and foodstuffs from the Succession States, and she would have, in addition, a great German area in which she would find scope for her commercial and financial activities. Even if Naumann were but playing the part of the Tempter, who said 'All these things will I give thee if thou wilt fall down and worship me,' he undoubtedly told the truth when he said 'The whole of Germany is now more open to the Viennese crafts than ever before. The Viennese might make an artistic conquest extending to Hamburg and Danzig.' But not only would Austria find a market for her industrial products in Germany, she would become the great trading centre between Germany and south-east Europe, and in that way would once more be, but in a newer and better sense than before, the *Ostmark* of the German people.

The absorption of Austria in Germany is opposed by France, mainly because she cannot conceive that her great secular struggle with the people on the other side of the Rhine will ever come to an end, and she fears the addition of 6,500,000 to the population of her ancient enemy. But quite apart from the fact that Germany and Austria cannot permanently be prevented from following a common destiny if they so desire, and apart from the fact that politically it is desirable they should do so with at least the tacit assent of the Allied Powers rather than in face of their avowed hostility, there are reasons for thinking that any danger to which France might be exposed by the additional man-power given to Germany would be more than compensated for by the altered political condition in Germany herself. Vienna would form an effective counterpoise to Berlin, and all the more so because she is a great geographical centre, while Berlin is more or less a political creation. The South German people have never loved the latter city, and to-day they love her less than ever. In Vienna they would find not only a kindred civilisation with which they would be in sympathy, but a political leadership to which they would readily give heed. In such a Germany, divided in its allegiance between Berlin and Vienna, Prussian animosity to France would be more or less neutralised. Nor would Germany suffer disproportionately to her gain, since in the intermingling of Northern efficiency with Southern culture she would find a remedy for much of the present discontents. When the time comes, and Austria seeks to ally herself with her kin, we hope that no impassable obstacle will be placed in her way.

The long and as yet unsettled controversy on the limits of the Italian Kingdom illustrates very well the difficulties which may arise when geographical and ethnical conditions are subordinated to considerations of military strategy, history, and sentiment in the determination of national boundaries. The annexation of the Alto Adige has been generally accepted as inevitable. It is true that the population is German, but here, as in Bohemia, geographical



conditions appear to speak the final word. Strategically also the frontier is good, and will do much to allay Italian anxiety with regard to the future. Hence, although ethnical conditions are to some extent ignored, the settlement which has been made will probably be a lasting one.

On the east the natural frontier of Italy obviously runs across the uplands from some point near the eastern extremity of the Carnic Alps to the Adriatic. The pre-war frontier was unsatisfactory for one reason because it assigned to Austria the essentially Italian region of the lower Isonzo. But once the lowlands are left on the west the uplands which border them on the east, whether Alpine or Karst, mark the natural limits of the Italian Kingdom, and beyond a position on them for strategic reasons the Italians have no claims in this direction except what they can establish on ethnical grounds. To these, therefore, we turn. In Carniola the Slovenes are in a large majority, and in Gorizia they also form the bulk of the population. On the other hand, in the town and district of Trieste the Italians predominate, and they also form a solid block on the west coast of Istria, though the rest of that country is peopled mainly by Slovenes. It seems to follow, therefore, that the plains of the Isonzo, the district of Trieste, and the west coast of Istria, with as much of the neighbouring upland as is necessary to secure their safety and communications, should be Italian and that the remainder should pass to the Jugo-Slavs. The so-called Wilson line, which runs from the neighbourhood of Tarvis to the mouth of the Arsa, met these requirements fairly well, though it placed from 300,000 to 400,000 Jugo-Slavs under Italian rule, to less than 50,000 Italians, half of whom are in Fiume itself transferred to the Jugo-Slavs. Any additional territory must, by incorporating a larger alien element, be a source of weakness and not of strength to Italy. To Fiume the Italians have no claim beyond the fact that in the town itself they slightly outnumber the Croats, though in the double town of Fiume-Sushak there is a large Slav majority. Beyond the sentimental reasons which they urge in public, however, there is the economic argument, which, perhaps wisely, they keep in the background. So long as Trieste and Fiume belonged to the same empire the limits within which each operated were fairly well defined, but if Fiume become Jugo-Slav it will not only prove a serious rival to Trieste, but will prevent Italy from exercising absolute control over much of the trade of Central Europe. For Trieste itself Italy has in truth little need, and the present condition of that city is eloquent testimony of the extent to which it depended for its prosperity upon the Austrian and German Empires. In the interests, then, not only of Jugo-Slavia but of Europe generally, Fiume must not become Italian, and the idea of constituting it a Free State might well be abandoned. Its development is more fully assured as the one great port of Jugo-Slavia than under any other form of government.

With regard to Italian claims in the Adriatic, little need be said. To the Dalmatian coast Italy has no right either on geographical or on ethnical grounds, and the possession of Pola, Valona, and some of the

islands gives her all the strategic advantages which she has reason to demand. But, after all, the only danger which could threaten her in the Adriatic would come from Jugo-Slavia, and her best insurance against that danger would be an agreement by which the Adriatic should be neutralised. The destruction of the Austro-Hungarian fleet offers Italy a great opportunity of which she would do well to take advantage.

Of the prospects of Jugo-Slavia it is hard to speak with any feeling of certainty. With the exception of parts of Croatia-Slavonia and of Southern Hungary, the country is from the physical point of view essentially Balkan, and diversity rather than unity is its most pronounced characteristic. From this physical diversity there naturally results a diversity in outlook which might indeed be all to the good if the different parts of the country were linked together by a well-developed system of communication. Owing to the structure of the land, however, such a system will take long to complete.

Ethnic affinity forms the real basis of union, but whether that union implies unity is another matter. It is arguable that repulsion from the various peoples—Magyars, Turks, and Austrians—by whom they have been oppressed, rather than the attraction of kinship, is the force which has brought the Jugo-Slavs together. In any case the obstacles in the way of the growth of a strong national feeling are many. Serb, Croat, and Slovene, though they are all members of the Slav family, have each their distinctions and characteristics which political differences may tend to exaggerate rather than obliterate. In Serbian Macedonia, again, out of a total population of 1,100,000, there are 400,000 to 500,000 people who, though Slavs, are Bulgarian in their sympathies, and between Serb and Bulgarian there will long be bitter enmity. Religious differences are not wanting. The Serbs belong to the Orthodox Church, but the Croats are Catholics, and in Bosnia there is a strong Mohammedan element. Cultural conditions show a wide range. The Macedonian Serb, who has but lately escaped from Turkish misrule, the untutored but independent Montenegrin, the Dalmatian, with his long traditions of Italian civilisation, the Serb of the kingdom, a sturdy fighter but without great political insight, and the Croat and Slovene, whose intellectual superiority is generally admitted, all stand on different levels in the scale of civilisation. To build up out of elements in many respects so diverse a common nationality without destroying what is best in each will be a long and laborious task. Economic conditions are not likely to be of much assistance. It is true that they are fairly uniform throughout Jugo-Slavia, and it is improbable that the economic interests of different regions will conflict to any great extent. On the other hand, since each region is more or less self-supporting, they will naturally unite into an economic whole less easily than if there had been greater diversity. What the future holds for Jugo-Slavia it is as yet impossible to say; but the country is one of great potentialities, and a long period of political rest might render possible the development of an important State.

This brings me to my conclusion. I have endeavoured to consider the great changes which have been made in Europe not in regard to the extent to which they do or do not comply with the canons of

boundary-making, for after all there are no frontiers in Europe which can in these days of modern warfare be considered as providing a sure defence, but in regard rather to the stability of the States concerned. A great experiment has been made in the new settlement of Europe, and an experiment which contains at least the germs of success. But in many ways it falls far short of perfection, and even if it were perfect it could not be permanent. The methods which ought to be adopted to render it more equable and to adapt it to changing needs it is not for us to discuss here. But as geographers engaged in the study of the ever-changing relations of man to his environment we can play an important part in the formation of that enlightened public opinion upon which alone a society of nations can be established.

# British Association for the Advancement of Science.

SECTION F: CARDIFF, 1920.

## ADDRESS TO THE SECTION OF ECONOMIC SCIENCE AND ~~STATISTICS~~

BY

J. H. CLAPHAM, C.B.E., LITT.D.,

PRESIDENT OF THE SECTION.

IT IS, I think, a President's first duty to record the losses which economic science has sustained since the Association last met. A year ago we had just lost, on the academic side, Archdeacon Cunningham, and on the side of affairs, Sir Edward Holden. This year, happily, I have no such losses to record in either field. But it is right to name the death of a late enemy, Professor Gustav Cohn, of Göttingen, an economist of the first rank, who had made a special study of English affairs. I believe that no student of our railway history would fail to place Cohn's *Inquiries into English Railway Policy*, published (in German) so long ago as 1873, first on the unfortunately very short list of scientific works devoted to that side of history. Even when supplemented by an additional volume, issued ten years later, it covers only what seems to-day the prehistoric period of our policy—before the Act of 1888 and very long before our present uncertainties—but it is not yet out of date. Cohn died full of years. He was nearly eighty. I may mention, perhaps, with his name that of a much younger, and possibly more brilliant, German economist, Max Weber, of Munich, who has died at the age of fifty-six. He once tried to explain, by a study of Puritan theology, the economic qualities of the Nonconformist business man—a very fascinating study. But his work as a whole has not roused much interest in England.

By an accident the three scholars whose names I have mentioned were all best known, in England at any rate, as historians. And, with your indulgence, I will do what I think has seldom been done from this chair, in making my address largely historical. History has been my main business in life; and it has occurred to me that some comparisons between the economic condition of Europe after the great wars of a century ago and its condition to-day may not be without interest. Historical situations are never reproduced, even approximately; but it is at least interesting to recall the post-war problems which our grandfathers or great grandfathers had to face, and how they handled them; to ask how far our sufferings and anxieties have had their parallels in the not remote past; and to note some danger

signals. By 'we' I mean not the British only, but all the peoples of Western and Central Europe. Of Eastern Europe I will only speak incidentally; for I am unable as yet to extract truth from the conflicting and biased evidence as to its economic condition. Moreover, there is still war in the East.

In 1815 France had been engaged in almost continuous wars for twenty-three, England for twenty-two, years. The German States had been at war less continuously; but they had been fought over, conquered, and occupied by the French. Prussia, for instance, was overthrown in 1806. When the final struggle against Napoleon began, in 1812, there was a French army of occupation of nearly 150,000 men in Prussia alone. From 1806 to 1814 Napoleon's attempt to exclude English trade from the Continent had led to the English blockade—with its striking resemblances to, and its striking differences from, the blockade of 1914-19. Warfare was less horribly intense, and so less economically destructive, than it has become in our day; but what it lacked in intensity it made up in duration.

Take, for instance, the loss of life. For England it was relatively small—because for us the wars were never people's wars. In France also it was relatively small in the earlier years, when armies of the old size were mainly employed. But under Napoleon it became enormous. Exact figures do not exist, but French statisticians are disposed to place the losses in the ten years that ended with Waterloo at fully 1,500,000. Some place them higher. As the population of France grew about 40 per cent. between 1805-15 and 1904-14, this would correspond to a loss of, say, 2,100,000 on the population of 1914. The actual losses in 1914-18 are put at 1,370,000 killed and missing; and I believe these figures contain some colonial troops.

Or take the debts accumulated by victors and the requisitions or indemnities extorted from the vanquished. The wars of a century ago left the British debt at \$48,000,000. According to our success or failure in securing repayment of loans made to Dominions and Allies, the Great War will have left us with a liability of from eight to nine times that amount. Whether our debt-carrying capacity is eight or nine times what it was a century ago may be doubted, and cannot be accurately determined. But it is not, I would venture to say, less than six or seven times what it was, and it might well be more. A good deal depends on future price levels. At least the burdens are comparable; and we understand better now where to look for broad shoulders to bear them.

After Waterloo France was called upon to pay a war indemnity of only 28,000,000*l.*, to be divided among all the victors. With this figure Prussia was thoroughly dissatisfied. Not, I think, without some reason. She reckoned that Napoleon had squeezed out of her alone, between 1806 and 1812, more than twice as much—a tremendous exaction, for she was in those days a very poor land of squires and peasants, whose treasury only received a few millions a year. England, who was mainly responsible—and that for sound political reasons—for the low figure demanded of France, found herself, the victor, in the curious position of being far more heavily burdened with debt than France, who

had lost. England, of course, had acquired much colonial territory; but on the purely financial side the comparison between her and France was most unequal. England's total national debt in 1817 was 848,000,000*l.* France's debt did not reach 200,000,000*l.* until 1830.

The reasons why France came out of the wars so well financially were four. *First*, she had gone bankrupt during the Revolution, and had wiped out most of her old debt. *Second*, under Napoleon she had made war pay for itself, as the case of Prussia shows. *Third*, there was no financial operation known to the world in 1815 by which England's war debt, or even half of it, could have been transferred to France. *Fourth*, England never suggested any such transference, or, so far as I know, ever even discussed it.

France's financial comfort, immediately after her defeat, extended to her currency. During the Revolution she had made a classical experiment in the mismanagement of credit documents, with the assignats issued on the security of confiscated Church property; but after that she had put her currency in good order. Her final defeat in 1812-14, and again in 1815, did not seriously derange it. Indeed, the English currency was in worse order than the French, owing to the suspension of cash payments by the Bank of England; and so rapidly did France's credit recover after 1815 that in 1818 French 5 per cents. stood at almost exactly the present-day price of British 5 per cent War Loan. That year she finished the payment of her war indemnity, and the last armies of occupation withdrew.

She had no doubt gained by waging war, and eventually suffering defeat, on foreign soil. No French city had been burnt like Moscow, stormed like Badajoz, or made the heart of a gigantic battle like Leipzig. Napoleon fought one brilliant defensive campaign on French soil, in the valleys of the Marne and the Seine, in 1814. In 1815 his fate was decided in Belgium. Hardly a shot was fired in France; hardly a French cornfield was trampled down. But France, as in 1918, was terribly short of men, and, again, as in 1918, her means of communication had suffered. Napoleon's magnificent roads—he was among the greatest of road engineers—had gone out of repair; his great canal works had been suspended. These things, however, were soon set right by the Government which followed him.

France's rapid recovery brings us to one of the essential differences between Western Europe a century ago and Western Europe to-day. In spite of Paris and her other great towns, the France of 1915 was a rural country, a land of peasants and small farmers. Only about 10 per cent. of her population lived in towns of 10,000 inhabitants or more. The town below 10,000, in all countries, is more often a rural market town, ultimately dependent on the prosperity of agriculture, than an industrial centre. Parallels for France's condition must be sought to-day in Eastern Europe—in Serbia or Russia. It is a condition which makes the economics of demobilisation easy. The young peasant goes back from the armies to relieve his father, his mother, and his sisters, who have kept the farm going. Moreover, France maintained a standing army of 240,000 men after 1815; and her losses in the Waterloo campaign had been so heavy that the actual numbers demobilised were

relatively small. Demobilisation left hardly a ripple on the surface of her economic life.

The German States were far more rural in character even than France. There were a few industrial districts, of a sort, in the West and in Saxony; a few trading towns of some size, like Hamburg and Frankfurt; but there was nowhere a city comparable to Paris. In 1819 the twenty-five cities which were to become in our day the greatest of the modern German Empire had not 1,250,000 inhabitants between them. Paris alone at that time had about 700,000. German statesmen, when peace came, were occupied not with problems arising from the situation of the urban wage-earner, though such problems existed, but with how to emancipate the peasants from the condition of semi-servility in which they had lived during the previous century. Here, too, demobilisation presented few of the problems familiar to us. Probably not one man in ten demobilised was a pure wage-earner. The rest had links with the soil. The land, neglected during the war, was crying out for labour, and every man had his place, even if it was a servile place, in rural society.

Things were different in England; but our demobilisation problem was smaller than that of our Continental allies or enemies, who had mobilised national armies, though not of the modern size. On the other hand, we had kept an immense fleet in commission, the crews of which were rapidly discharged. Early in 1817 Lord Castlereagh stated in Parliament that 300,000 soldiers and sailors had been discharged since the peace. In proportion to population, that would be equivalent, for the whole United Kingdom, to nearly 750,000 to-day. For these men no provision whatever was made. They were simply thrown on the labour market; and the vast majority of them were ex-wage-earners or potential wage-earners, industrial, mercantile, or agricultural. The United Kingdom was not urbanised as it is to-day; but the census of 1821 showed that 21 per cent. of the population lived in cities of 20,000 inhabitants and upwards, and probably about 27 per cent. (as compared with France's 10 per cent.) lived in places of 10,000 and upwards. As industry in various forms, especially coal-mining, spinning, and weaving, was extensively carried on in rural or semi-rural districts, it is certain that at least one demobilised man of working age in every three was a potential wage-earner of industry or commerce. And as Great Britain had lost most of her peasant-holders, whether owners or small working farmers, the remainder of the demobilised rank and file were nearly all of the agricultural labourer class. They had to find employment: there was not a place in rural society waiting for them, as there was for the average French or German peasant soldier. It is not surprising that the years from 1815 to 1820 were, both economically and politically, probably the most wretched, difficult, and dangerous in modern English history.

Things were at their worst in 1816-17, both for England and for her continental neighbours. Western Europe was very near starvation. Had the harvest of 1815 not been excellent, so providing a carry-over of corn, or had the harvest of 1817 been much below the average, there must have been widespread disaster; so thorough and universal

was the harvest failure of 1816. In the latter part of 1815 (December) wheat fell in England to 55s. 7d., although no grain imports were allowed, except of oats. Early in 1816 the United Kingdom was actually exporting a little wheat. Then came a terrible spring—a long frost; snow lying about Edinburgh in May; all the rivers of Western Europe in flood. An equally disastrous summer followed. There was dearth, in places amounting to real famine, everywhere—worst of all in Germany. Unlike France, the German States of a century ago were extraordinarily ill-provided with roads. What roads there were had gone to pieces in the wars. In winter even the mails could hardly get through with sixteen and twenty horses. Food supplies could not be moved over long distances by land; and the slightly more favoured regions could not help the most unfortunate. There was a far wider gap between prices in Eastern and Western Germany in 1816 than there had been in the last bad famine year (1772). Each German State, in its anxiety, began to forbid export early in 1816, thus making things worse. At Frankfurt, the representatives of the German States, gathered for the Diet, could hardly feed their horses. Prices rose amazingly and quite irregularly, with the varying food conditions of the various provinces. In the spring of 1817 pallid half-starved people were wandering the fields, hunting for and grubbing up overlooked and rotten potatoes of the last year's crop.

In England the harvest failure of 1816 drove wheat up to 103s. 7d. a quarter for December of that year, and to 112s. 8d. for June of 1817. In Paris the June price in 1817 was equivalent to 122s. 5d. At Stuttgart the May price was equivalent to 138s. 7d. These are only samples. Think what these figures mean at a time when an English agricultural labourer's wage was about 9s. 6d., and a French or German unskilled wage far less. It must be recalled that there were no special currency causes of high prices either in France or Germany. These were real dearth prices. In the spring of '17 the French Government was buying corn wherever it could find it—in England, North Africa, America—as another bad harvest was feared. Happily, the 1817 harvest was abundant, here and on the Continent. By September the Mark Lane price of wheat was 77s. 7d., and the Paris price 71s. 9d.

I have gone into price details for the purpose of drawing a contrast between a century ago and to-day. Except for the damage done to the German roads, the wars had very little to do with these food troubles of 1816-17. High and fluctuating food prices were the natural consequence of the general economic position of Western Europe a century ago. It was only in the most comfortable age in all history—the late nineteenth and early twentieth centuries—that low and stable food prices came to be regarded as normal. In the eighteenth century, when England fed herself and often had an exportable surplus, fluctuations were incessant. Take the ten years 1750-1760. The mean price of wheat at Eton in '52 was 45 per cent. above the mean price in '50. The mean price in '57 was nearly 100 per cent. above the mean price of '50. On Lady Day '57 the price was 60s. 5½d. On Lady Day '59 it was 37s. 4d. On Lady Day '61 it was 26s. 8d. The '61 mean price was exactly half the '57 mean price.



Eighteenth-century England was too well organised economically to be in much risk of actual famine, but for Ireland and large parts of the Continent famine was a normal risk. War and its effects had only accentuated, not created, that risk. Imports might reduce it, but could not avert it, because Western Europe tends to have approximately the same harvest conditions throughout, and it was impossible to draw really large supplementary supplies from anywhere else. So unimportant were overseas supplies that the Continent suffered very much more from the harvest failure of '16, in time of peace, than from the eight years' English blockade in time of war. If overseas supplies could be got they were hard to distribute, owing to defective transport facilities. Thanks to the work of the nineteenth century, the most terrific of all wars was required to bring Western Europe face to face with what had been both a war-time and a peace-time risk a century earlier.

But the old Europe, if it had the defects, had also the elasticity of a rather primitive economic organism. Given a couple of good harvests, and a land of peasants soon recovers from war. Serbia had a good harvest last year (1919), and was at once in a state of comparative comfort, in spite of her years of suffering. A second good harvest this year, for which fortunately the prospects are favourable, would almost restore her. So it was with France and, to a less extent, Germany in 1816-18. In France acute distress in 1816-17 had been confined to the towns and to those country districts where the harvest failure was worst. The harvest of '17 put an end to it. One gets the impression that in Germany distress among the peasants themselves had been more widespread. Worse communications and the absence of a strong central Government seem to have been the chief causes of this, though perhaps the harvest failure was more complete. In France, as we have seen, the central Government took such action as was possible in the interests of the whole country. A parallel might be drawn between the German situation in 1815-17 and that of the States which have arisen from the break-up of the old Austro-Hungarian Empire since 1918. Freed from French domination, and then from the urgent necessity of co-operating against a common enemy, the German States relapsed into their ancient jealousies and conflicting economic policies, just as the new States, which were once subject to the Hapsburgs, have been forbidding exports of food and fuel and disputing with one another.

An excellent harvest in 1817 averted the risk of famine in Germany also; but anything that could be called prosperity was long delayed, whereas France was indisputably prosperous, judged by the standards of the day, and far more contented than England, by 1818-20. Germany had been so exhausted by the wars and incessant territorial changes of the Napoleonic age, and was politically so divided, that her economic life remained stagnant and her poverty great until at least 1830. It was all that the various Governments could do to find money for the most essential of all economic measures—the repair and construction of roads—whereas France had her splendid main roads in order again and had resumed work on her canals before 1820. But France had

cut her losses nearly twenty years before, and had enjoyed continuous freedom from war on her own territory between 1794 and 1814, as we have seen. She had been well, if autocratically, governed, and her war indemnity was but a trifling burden. Her peasants were free and, as a class, vigorous and hopeful. She was united and conscious of her leadership in Europe, even through her ultimate defeats.

If the experience of Europe after Waterloo is, on the whole, of good augury for agricultural States, and especially for agricultural States with a competent central Government, for the industrialised modern world that experience is less encouraging. Great Britain alone was partially industrialised in 1815-20, and Great Britain, though victorious, suffered acutely. Mismanagement was largely responsible for her sufferings—mismanagement of, or rather complete indifference to, problems of demobilisation; mismanagement of taxes (the income tax was abandoned at the clamour of interested parties, and the interest on the huge debt paid mainly from indirect taxes, which bore heavily on the poor); mismanagement of food supplies, by the imposition of the Corn Law; and so on. But suffering due to international economic dislocation following war could not have been avoided by management, however good. The situation was unique. England alone of the European Powers had developed her manufactures to some extent on what we call modern lines. During the wars she had accumulated also great stores of colonial and American produce, which could only get into Europe with difficulty—by way of smuggling. In 1813, before Napoleon's first fall, her manufacturers and merchants were eagerly awaiting peace. In 1814 manufactures and colonial produce were rushed over, only to find that, much as Europe desired them, it could not pay the price. It had not enough to give in exchange; and England, being rigidly protectionist, was not always prepared to buy even what Europe had to give. There was no machinery for international buying credits. Merchants shipped at their own risks, usually as a venture, not against a firm order as to-day, and they had to bear their own losses—often up to 50 per cent. Continental economic historians have hardly yet forgiven us for this 'dumping,' which both drained away the precious metals to England—as there was not much else to pay with—and did a great deal of harm to the struggling young factory industries, which had begun to grow up under the protection of Napoleon's anti-English commercial policy.

British exporters were so badly bitten in 1814 that, when peace finally came next year, after Waterloo, they were nervous of giving orders at home—which was very bad for the manufacturing industries and for the men who sought employment in them. There was the curious situation in 1816 that, while the price of wheat was rushing up, most other prices were falling, the bottom of the market being often reached at the end of the year, when the confidence of buyers and shippers began to revive. Raw cotton, for instance, which had touched 2s. 6d. a lb. in 1813-14, fell to a minimum of 1s. 2d. in 1816—although Europe was open and cotton badly needed.

It is as yet too early to work out a parallel between this post-war commercial and industrial slump and the slump that followed the Great

War of 1914-18, for we have not yet had it. But it is coming. More certainly, I am inclined to believe, in the United States than in England; but pretty certainly here also. I say more certainly in the United States because her position bears most resemblance to that of England in 1815-17. Consider that position. What before the war was, on the balance, a debtor country has become a creditor country. That creditor is equipped to export both raw materials and manufactures—iron and steel goods particularly—on a huge scale. It is true she is a heavy importer of some foods, such as sugar, coffee, and tea, and of certain raw materials, such as rubber, timber, and wool. But, owing to her tariff system and her general policy, she is reluctant to take many things which her debtors have to offer. Her recent 'dry' policy, for example, has shut her markets to one of France's most valuable exports, an export with which France has always been in the habit of paying her creditors. Already, I notice, American business men are beginning to point out what English business men stated clearly in a famous document, the Petition of the London Merchants, a century ago—that the country which will not buy, neither shall it sell. This was the most solid of all free-trade arguments in the early nineteenth century, and it has lost none of its force. No doubt America is, and will be, glad to take part payment in gold, just as England was in 1814-16. But that is not a permanent solution. If she remains a creditor nation—and there is no present reason to think that she will not—she must in time arrange to take more goods from outside. Her political processes, however, are slow; and it seems unlikely that she will have adjusted her policy before the post-war slump is upon her.

The United Kingdom, which, on the whole, still takes freely what its customers have to offer it, is in a better position, provided its customers can go on offering. This may prove an important proviso. Customers who have been little hurt or even helped by the war—Spain, perhaps, or Egypt, or India, or New Zealand—should continue good buyers. But the uncertainty gives cause for anxious thought in the case of the war-damaged nations, allied and ex-enemy. Modern financial and commercial organisation have postponed the critical moment in a way that was impossible a century ago. When Europe was hungry in 1816 there were not food surpluses available anywhere on the earth, nor shipping enough on the seas, nor means of transport good enough on land, to relieve her need. If, *per impossibile*, there had been all these things, there would have been no country or group of business men anywhere ready to give her the necessary credit on a large scale. The Rothschilds, a young firm in those days, did something. They advanced money to a few German princes to buy corn for their people at the Baltic ports, for there was some corn to spare from Poland and Russia. But the huge food-financing operations of 1918-20 would have been as unthinkable as the actual handling of the foodstuffs would have been impossible. Had two harvests like that of 1816 come in succession, there would have been famine and food riots everywhere, past hope of cure.

\*Similarly modern finance is postponing the critical moment for the

international trade in manufactures. British business men in 1919-20 have not, I believe, often sent their goods abroad in hope of finding a vent for them, and then been forced to content themselves with prices far below cost of production, as their grandfathers were in 1814-16. Every kind of financial device—long private credit, assistance from banks, credits given by Governments—has been called in, so that trade may be resumed before the war-damaged nations are in a position to pay for what they need by exporting the produce of their own labour. The more industrial the damaged nation is, the more complex is the restarting of her economic activity. Corn grows in nine months, and pigs breed fast. The start once given, countries like Denmark and Serbia, both of which are normally great exporters of pigs or bacon, could soon pay for necessary imports of machinery or fertilisers bought on long credit to restart their rural industries. The United Kingdom, the least damaged of all the combatants except America, is believed by the Chancellor of the Exchequer to be now rather more than paying its way. That may be sanguine, but at the worst our accounts are nearly balanced. What might not have happened in 1919 if modern methods for postponing payment had not been applied internationally? The other chief combatants are far from paying their way. Italy is importing abnormal quantities of food and also her necessary raw materials with the aid of American and English credits, while Germany, who can get little in the way of credit, has hardly begun even to import the raw materials to make the goods by the export of which she may eventually pay her way, not to mention her indemnities. I have in mind such materials as cotton, wool, rubber, copper, oil-seeds, and hides—all of which she imported heavily in 1913. Some materials, of course, she possesses in abundance, but the working up even of these is hampered by her coal position. I make no political pleas: I merely illustrate the complexity of the restarting of industry under present-day conditions. France has the first claim to assistance in restarting, a claim which we all recognise; but for the comfort and peace of the world a universal restart is desirable.

The central problem is one which I can only indicate here, not discuss. Its discussion is for experts with full inside knowledge from month to month, and the answer varies for every country. It is—when will the inability of the war-damaged nations to pay for all that they want, in food and materials, in order to restart full economic activity, make itself felt by the nations who are supplying them, primarily, that is, the United States and ourselves? In 1814-16, when the problem was, of course, infinitely smaller because nations were so much more self-sufficing, the reaction came at once for lack of long organised credits. Conceivably, all other combatants might do in turn what we seem to have done—that is, adjust their trade balance within a reasonable period and so avoid renewal of special credits. In that case the post-war trade slump would come, not as an international crisis, but as a gradual decline, when the first abnormal demand for goods of all kinds to replenish stocks is over. Already this type of demand is slackening in certain quarters. We shall do very well if we have nothing worse than that gradual decline, which would be eased, in our case, by our

extensive connections with undamaged countries, and by our willingness to buy most things which any nation has to offer. The situation would be still further eased if countries such as Germany and Russia were to develop in turn what might be called a reconstruction demand, to take the place of the satisfied reconstruction demands of our Allies. But the fear, as I think the quite reasonable fear, expressed in some well-informed quarters, is that, in view of the complicated and dangerous currency position in many countries; in view of the difficulty which the war-damaged nations have in collecting taxes enough to meet their obligations; in view of the slowness with which some of them are raising production to the level of consumption; in view of the complete uncertainty of the political and economic future in much of Central and Eastern Europe—that in view of these things, and quite apart from possible political disturbances, we shall have to go through a genuine crisis, as distinct from a depression; a crisis beginning in the field of finance, when some international obligation cannot be met or some international credit cannot be renewed, spreading to industry and giving us a bad spell of unemployment, comparable with the unemployment of the post-war period a century ago, and more dangerous because of the high standard of living to which the people in this and some other countries is becoming accustomed.

Personally, I am less apprehensive for the industrious of this country than are many, whose opinions I should ordinarily be disposed to prefer to my own. A demand, an effective demand, exists for many things that we can supply in great regions outside the war area—in China, for instance, where there is said to be at this moment a keen demand for machinery which the United Kingdom is too much preoccupied with other work to supply. Nor do I fear that a crisis will originate here: as I am disposed to think that our currency and taxation position is already relatively sound. But we should be bound to feel the reactions of a crisis which might occur elsewhere; to what extent is, however, quite impossible to foresee.

One final comparison. An extraordinary feature of the great wars of a century ago was that they coincided with a steady growth of population, and were followed by a period of rapid growth. For the United Kingdom that fact is well known and not surprising. We lost relatively few men in war. But the official French figures, 27,500,000 in 1801 and 29,500,000 in 1816, are so remarkable that one is tempted to doubt the first enumeration. Though remarkable, the figures are, however, not impossible; and it must be recalled that the losses were spread over many years. British population has grown a little since 1914; in spite of separations of man and wife and our three-quarters of a million dead. A main reason has, however, been the suspension of emigration, which was proceeding at a rate of over 200,000 a year just before the war. France estimates a dead loss of over 3,000,000 (on 39,700,000) between 1913 and 1918 on her old territory. Her census is due next year. Comparatively early in the war the German *civilian* death rate was above the birth rate; so presumably she is in much the same position as France. But, owing to changes of frontier and continued unrest, it is as yet too early to estimate the total

effect of the Great War on population. For Western and Central Europe it must, I think, have produced a considerable net loss. For Russia one can hardly guess; but her population is so largely rural and grew so amazingly fast before 1914, that it would not surprise me very much to learn that, with all her miseries, it had been maintained.

The growth of population in Europe after 1815 coincided with the spread of the first industrial and agricultural revolution outwards from the United Kingdom. The world was learning new ways to feed and clothe itself; and it continued to learn all through the century. I myself do not suppose that the age of discovery is at an end, so our troubles may be eased as time goes on; and although I have not the slightest wish that population should ever again grow so fast as it grew in Europe during the nineteenth century, I see no reason why a moderate rate of growth should not be resumed, in a few years at latest. But perhaps I have already committed prophecy, or half prophecy, more than is altogether wise for one in my position.



# British Association for the Advancement of Science.

SECTION G : CARDIFF, 1920.

## ADDRESS TO THE ENGINEERING SECTION BY

PROFESSOR C. F. JENKIN, C.B.E., M.A.,  
PRESIDENT OF THE SECTION.

THE importance of research in all branches of industry is now becoming fully recognised. It is hardly necessary to point out the great possibilities of the Board of Scientific and Industrial Research, formed just before the war, or to lay stress on the attention which has been called to the need for research by events during the war. Probably in no branch of the Services was more research work done than in the Air Service, and the advances made in all directions in connection with flying were astonishing. My own work was confined to problems connected with materials of construction, and as a result of that work I have come to the conclusion that the time has come when the fundamental data on which the engineering theories of the strength and suitability of materials are based require thorough overhauling and revision. I believe that the present is a favourable time for this work, but I think that attention needs to be drawn to it, lest research work is all diverted to the problems which attract more attention, owing to their being in the forefront of the advancing engineering knowledge, and lest the necessary drudgery is shirked in favour of the more exciting new discoveries.

It has been very remarkable how again and again in aeroplane engineering the problems to be solved have raised fundamental questions in the strength and properties of materials which had never been adequately solved. Some of these questions related to what may be termed theory, and some related to the physical properties of materials. I propose to-day to describe some of these problems, and to suggest the direction in which revision and extension of our fundamental theories and data are required and the lines on which research should be undertaken. Let us consider first one of the oldest materials of construction—timber. Timber was of prime importance in aircraft construction. The first peculiarity of this material which strikes us is that it is anisotropic. Its grain may be used to locate three principal axes—along the grain, radially across the grain, and tangentially across the grain. It is curious that there do not appear to be generally recognised terms for these three fundamental directions. A very few



tests are sufficient to show that its strength is enormously greater along the grain than across it. How, then, is an engineer to calculate the strength of a wooden member? There is no theory, in a form available for the engineer, by which the strength of members made of an anisotropic material can be calculated.

I fancy I may be told that such a theory is not required—that experience shows that the ordinary theory is quite near enough. How utterly misleading such a statement is I will try to show by a few examples. Suppose a wooden tie or strut is cut from the tree obliquely so that the grain does not lie parallel to its length. In practice it is never possible to ensure that the grain is accurately parallel to the length of the member, and often the deviation is considerable. How much is the member weakened? This comparatively simple problem has been of immense importance in aeroplane construction, and, thanks to the researches made during the war, can be answered. The solution has thrown a flood of light on many failures which before were obscure. If the tensile strengths of a piece of timber are, say, 18,000 lb./sq. in. along the grain and 800 lb./sq. in. across it (radially or tangentially) and the shear strength is 900 lb./sq. in. along the grain—these figures correspond roughly with the strengths of silver spruce—then if a tensile stress be applied at any angle to the grain the components of that stress in the principal directions must not exceed the above strengths, or failure will occur. Thus we can draw curves limiting the stress at any angle to the grain, and similar curves may be drawn for compression stresses. These theoretical curves have been checked experimentally, and the results of the tests confirm them closely, except in one particular. The strengths at small inclination to the grain fall even faster than the theoretical curves would lead us to expect. The very rapid drop in strength for quite small deviations is most striking.

Similar curves have been prepared for tensile and compressive stresses inclined in each of the three principal planes for spruce, ash, walnut, and mahogany, so that the strengths of these timbers to resist forces in any direction can now be estimated reasonably accurately.

As a second example consider the strength of plywood. Plywood is the name given to wood built up of several thicknesses glued together with the grain in alternate thicknesses running along and across the plank. The result of this crossing of the grain is that the plywood has roughly equal strength along and across the plank. Plywood is generally built up of thin veneers, which are cut from the log by slicing them off as the log revolves in a lathe.

Owing to the taper in the trunk of the tree and to other irregularities in form, the grain in the veneer rarely runs parallel to the surface, but generally runs through the sheet at a more or less oblique angle. As a consequence the strength of plywood is very variable, and tests show that it is not possible to rely on its having more than half the strength it would have if the grain in the veneers were not oblique. It is therefore obviously possible to improve the manufacture enormously by using veneers *split off*, following the grain, in place of the present sliced veneers. The superiority of split or riven wood over cut wood has been recognised for ages. I believe all ladders and ladder

rungs are riven. Hurdles, hoops, and laths are other examples. Knees in ships are chosen so that the grain follows the required outline.

Owing to the enormous difference in strength in timber along and across the grain, it is obviously important to get the grain in exactly the right direction to bear the loads it has to carry. The most perfect example I ever saw of building up a plywood structure to support all the loads on it was the frame of the German Schutte-Lanz airship, which was made entirely of wood. At the complex junctions of the various girders and ties the wood, which was built up of very thin veneers—hardly thicker than plane shavings—layers were put on most ingeniously in the direction of every stress.

During the war I have had to reject numerous types of built-up struts intended for aeroplanes, because the grain of the wood was in the wrong direction to bear the load. The example shown—a McGruer strut—is one of the most elegant designs, using the grain correctly.

Many of the tests applied to timber are wrong in theory and consequently misleading. For example, the common method of determining Young's modulus for timber is to measure the elastic deflection of a beam loaded in the middle and to calculate the modulus by the ordinary theory, neglecting the deflection due to shear, which is legitimate in isotropic materials; but in timber the shear modulus is very small—for example, in spruce it is only about one-sixtieth of Young's modulus—and consequently the shear deflection becomes quite appreciable, and the results obtained on test pieces of the common proportions lead to errors in the calculated Young's modulus of about 10 per cent.

The lantern plates show three standard tests; the first is supposed to give the shearing strength of the timber, but these test pieces fail by tension across the grain—not by shearing. Professor Robertson has shown that the true shear strength of spruce is about three times as great as the text-book figures, and has designed a test which gives fairly reliable results. The second figure represents a test intended to give the mean strength across the grain, but the concentration of stress at the grooves is so great that such test pieces fail under less than half the proper load. This fact was shown in a striking manner by narrowing a sample of this shape to half its width, when it actually bore a greater total load—*i.e.* more than double the stress borne by the original sample. The third figure represents a test piece intended to measure the rather vague quality, 'strength to resist splitting.' The results actually depend on the tensile strength across the grain, on the elastic constants, and on the accidental position of the bottom of the groove relatively to the spring or autumn wood in the annular rings. Unless the theory is understood, rational tests cannot be devised.

There are some valuable tropical timbers whose structure is far more complex than that of our ordinary northern woods. The grain in these timbers grows in alternating spirals—an arrangement which at first sight is almost incredible. The most striking example of this type of wood I have seen is the Indian 'Poon.' The sample on the table has been split in a series of tangential planes at varying distances from the centre of the tree, and it will be seen that the grain at one depth is growing in a right-hand spiral round the trunk; a little farther out

it grows straight up the trunk; further out again it grows in a left-hand spiral, and this is repeated again and again, with a pitch of about two inches. The timber is strong and probably well adapted for use in large pieces—it somewhat resembles plywood—but it is doubtful whether it is safe in small pieces. No theory is yet available for estimating its strength, and very elaborate tests would be needed to determine its reliability in all positions. I had to reject it for aeroplanes during the war for want of accurate knowledge of its properties.

These examples show how necessary it is to have a theory for the strength of anisotropic materials before we can either understand the causes of their failure or make full use of their properties or even test them rationally.

The second material we shall consider is steel, and in dealing with it I do not wish to enter into any of the dozen or so burning questions which are so familiar to all metallurgists and engineers, but to call your attention to a few more fundamental questions. Steel is not strictly isotropic—but we may consider it to be so to-day. The first obvious question the engineer has to answer is, 'What is its strength?' The usual tests give the Ultimate Strength, Yield Point, Elastic Limit, the Elongation, the Reduction of Area, and perhaps the Brinell and Izod figures. On which of these figures is the dimension of an engine part, which is being designed, to be based? If we choose the Ultimate Strength we must divide it by a large factor of safety—a factor of ignorance. If we choose the Yield Point we must remember that none of the higher-grade steels have any Yield Point, and the nominal Yield Point depends on the fancy of the tester. This entirely imaginary point cannot be used for accurate calculation except in a very few special cases. Can we base our calculation on the Elongation—the Reduction of Area—the Izod test? If we face the question honestly we realise that there is no known connection between the test results and the stress we can safely call on the steel to bear. The only connecting link is that cloak for our ignorance—the factor of safety.

I feel confident that the only reliable property on which to base the strength of any engine part is the suitable *Fatigue Limit*. We have not yet reached the position of being able to specify this figure, but a considerable number of tests show that in a wide range of steels (though there are some unexplained exceptions) the Fatigue Limit for equal  $\pm$  stresses is a little under half the Ultimate Strength, and is independent of the Elastic Limit and nominal Yield Point, so that the Ultimate Strength may be replaced as the most reliable guide to true strength, with a factor—no longer of ignorance, but to give the fatigue limit—of a little over 2.

If the Fatigue Limit is accepted as the only sound basis for strength calculation for engine parts, and it is difficult to find any valid objection to it, then it is obvious that there is urgent need for extensive researches in fatigue, for the available data are most meagre. The work is laborious, for there is not one Fatigue Limit, but a continuous series, as the signs and magnitudes of the stresses change. Many problems in connection with fatigue are of great importance and need much fuller

investigation than they have so far received—*e.g.*, the effect of speed of testing; the effect of rest and heat treatment in restoring fatigued material; the effect of previous testing at higher or lower stresses on the apparent fatigue limit of a test piece. Some observers have found indications that the material may possibly be strengthened by subjecting it to an alternating stress below its fatigue limit, so that the results of fatigue tests may depend on whether the limit is approached by increasing the stress or by decreasing it.

Improved methods of testing are also needed—particularly methods which will give the results quickly. Stromeyer's method of measuring the first rise of temperature, which indicates that the fatigue limit is passed, as the alternating load is gradually increased, is most promising; it certainly will not give the true fatigue limit in all cases, for it has been shown by Bairstow that with some ranges of stress a finite extension occurs at the beginning of a test and then ceases, under stresses lower than the fatigue limit. But the fatigue limit in that case would not be a safe guide, for finite changes of shape are not permissible in most machines, so that in that case also Stromeyer's test may be exactly what is wanted. It can probably be simplified in detail and made practicable for commercial use. Better methods of testing in torsion are also urgently needed, none of those at present used being free from serious defects. Finally, there is a fascinating field for physical research in investigating the internal mechanism of fatigue failure. Some most suggestive results have already been obtained, which extend the results obtained by Ewing.

For members of structures which are only subjected to steady loads I suggest that the safe stress might be defined by limiting the corresponding permanent set to a small amount—perhaps  $\frac{1}{2}$  per cent. or  $\frac{1}{4}$  per cent. This principle has been tentatively adopted in some of the aircraft material specifications by specifying a Proof Load which must be sustained without a permanent extension of more than  $\frac{1}{2}$  per cent. Whether this principle is suitable for all materials and how it will answer in practice remains to be proved by experience. It is at any rate a possible rational basis for determining the useful strength of a material under steady loads.

The relation between the proof stress and the shape of the stress-strain diagram is shown in the lantern slide. The curve is the record of an actual test on a certain copper alloy. If a length A B corresponding to  $\frac{1}{2}$  per cent. elongation be set off along the base line and a line B P be drawn through the point B parallel to the elastic line, to cut the curve in P, then the stress at P is the stress which will give  $\frac{1}{2}$  per cent. permanent set. Though  $\frac{1}{2}$  per cent. may appear rather a large permanent set to allow it will be seen from the figure that it is less than the elastic elongation would have been at the same stress, and we do not usually find elastic elongations serious.

As a commercial test the proof load is very easily applied. For this alloy the specified proof load is shown by the horizontal line so labelled. This load is to be applied and released, and the permanent extension is required by the specification to be less than  $\frac{1}{2}$  per cent. This sample passes the test easily. On the figure the condition for complying with

the specification is that the curve shall fall above Q. But the test does not require the curve to be determined.

If we admit that the fatigue limit is the proper basis for engine-strength calculations, there are a number of interesting modifications required in the common theory of the strength of materials. It will no longer be possible to neglect, as has been so general in the past, the uneven distribution of stress in irregularly shaped parts of machines. It has been generally recognised that sharp corners should be avoided when possible, but no theory is available to enable the stresses at corners to be calculated or to enable their effect on the strength of the member to be estimated. If fatigue is the critical factor in failure under fluctuating stresses such theory is most necessary. Even the roughest guide would be of great value. The nature and magnitude of the concentrations of stress which occur in practice have been investigated experimentally by Professor Coker by his elegant optical method which has given most valuable results, some of which are already being used in designing offices. If the mathematical theory is too difficult, it may be possible to lay down practical rules deduced from such experimental results—but the method still has many limitations, perhaps the most serious being that it can only be used on flat models. I believe Professor Coker expects to be able to extend the method to round models.

As a simple example to show the importance of the subject let us consider the effect of a groove round a straight round bar subject to alternating tension and compression—such a groove as a screw thread. There will be a concentration of stress at the bottom of the groove. The ratio of the stress at the bottom of a groove to the mean stress in the bar has been worked out mathematically by Mr. A. A. Griffith, and his calculations have been confirmed experimentally by his elegant soap-bubble method. The ratio depends on the relation between the depth of the groove, the radius at the bottom, and slightly on the angle between the sides. For a Whitworth form of thread the ratio will be about 3. If the Fatigue Limit is exceeded at the bottom of the groove the metal will fail and a minute crack will form there; this crack will soon spread right across the bar and total failure will result. Thus we see that the safe mean stress in the bar will be reduced to one-third what a plain bar will bear. The truth of this theory regarding the importance of concentrations of stress has still to be proved experimentally; if true, it is of far-reaching importance, since it applies to all concentrations of stress in machine parts subject to fluctuating loads.

The theory does not apply to steadily loaded members; in these the local excess of stress is relieved by the stretching of the minute portion which is overloaded, and no further consequences follow.

The theory appears to apply to grooves however small, and has an important bearing on the smoothness of the finish of machine parts. The surface of any engine part finished by filing is certainly entirely covered with scratches. Emery likewise leaves the surface scratched—though the scratches are smaller. If, however, polishing be carried further the surface may ultimately be freed from scratches and left in a burnished condition. In this condition amorphous metal has been smeared over the surface—the smooth appearance is not simply due

to the scratches being too small to see. The strength—under alternating stresses—appears to depend on the form of the scratches, and if the ratio of radius at the bottom of the scratch to its depth is fairly large, very little weakening occurs. It seems probable in the ordinary engineering finish produced by emery and oil that the scratches are broad and shallow. This subject is being investigated. A considerable amount of evidence has been collected from practical experience pointing to the important effect which a smooth finish has on the strength of heavily stressed engine parts.

Fatigue is probably the cause of failure of wires in wire ropes. A good deal of valuable experimental work has been done on the life of ropes, but so far as I am aware there is no satisfactory theory of their strength. This subject also requires research, and it seems probable that valuable practical results might follow if the true explanation of the cause of the breakages of the wires was determined.

These are only examples, but they may be sufficient to show how much work both experimental and theoretical requires to be done to give the engineer a really sound basis for the simplest strength calculations on any moving machinery. But there are more fundamental questions still which must be tackled before the simplest questions of all which meet the engineer can be answered scientifically. The two most urgent and most important questions which I met with during the war in connection with aircraft were always the same—Why did some part break? and, What is the best material to use for that part? It was most disconcerting to find how inadequate one's knowledge was to answer these two simple questions. The common answers are: To the first: 'It broke because it was too weak, make it stronger,' and to the second: 'General practice indicates such a material as the best—better not try any other or you may have trouble.' In aircraft weight is paramount, and to make a part stronger—*i.e.*, heavier—had to be the last resort, and when used was almost a confession of failure. 'General practice' was no guide in aeroplane engines, which are built of the strangest materials. The origins of fractures were traced to many causes, often lying far away from the site of the breakage; but with these I am not concerned to-day. I wish to confine our consideration to the actual fracture and to ask, 'What stress caused the fracture?' and 'What property of the metal was absent which would have enabled it to withstand that stress?' And again, 'What other material possesses suitable properties to withstand the stresses better?' These are the fundamental questions which I have referred to—and which urgently need answers.

As an example I will take a broken propeller shaft. It has broken in a beautiful spiral fracture. What stress causes that? I have failed to explain it by any of the facts I know about the steel it is made of. It is, of course, a fatigue fracture—*i.e.*, it spread gradually. The questions to be answered are, Did it fail under tension, bending or torsion? and, Why was a spiral direction followed by the failure as it spread?

It may be objected that the question is unimportant. I think not. For example, till we can determine the nature of the stress we cannot

indicate the nature of the load—thus I cannot say if it broke under a torsional load (possibly torsional vibration) or under a bending load (possibly due to some periodic variation of thrust on one of the propeller blades as it passed an obstruction). Until the nature of the load which caused the failure is known, it is very difficult to take steps to guard against similar accidents. For the most urgent reasons, therefore, we require to be able to understand the fracture, as in nearly all aircraft problems men's lives hang on the answer.

Turning now to the question of the most suitable material, I will take as an example the material for the crankshaft of an aeroplane engine. A few months before the Armistice there were difficulties in getting sufficient supplies of the high-grade nickel-chrome steel forgings then in general use for shafts, and proposals were made to use a plain carbon steel. Such a steel would be about 30 per cent. weaker, according to the ordinary tests. A conference of leading metallurgists and engineers was held to discuss the suggestion. No one present ventured to predict whether the weaker steel would answer or not, or whether the dimensions would have to be increased or not. It was pointed out that a French engine was now using 50-ton steel with better results than when using the 100-ton steel for which it was designed, no changes in dimensions having been made. Such a reduction of strength might be understood in ordinary engineering where there are large margins of safety, but in an aeroplane engine, in which every ounce of metal is cut off which can be spared, they show how completely ignorant engineers are of what the suitability of material depends on.

As another example, Why are oxygen cylinders annealed—repeatedly? Annealing reduces the steel to its weakest condition. I believe the fondness for annealing is due to our ignorance of the properties we require. Perhaps the quality of steel which an engineer fears most is brittleness. He believes that annealing will soften it and reduce the brittleness; so he anneals, blindly. The fact is that we do not know what brittleness is—we cannot define it—we cannot measure it—though there are endless empirical tests to detect it. Till we know what it means and can measure it we are in a miserable position. During the war I was consulted on what could be done to reduce the enormous weight of oxygen cylinders, and I advised that experiments should be made on the high-quality alloyed steel tubes we were using in aircraft construction. The department dealing with these tubes took the matter up, and alloyed steel cylinders, properly heat-treated, were made. These were, I believe, a success, and only weighed a small fraction of the old-fashioned cylinders. But my suggestion was little more than a guess, and no means was known of accurately testing the suitability of the material, so they were only accepted after passing any number of empirical tests, consisting of various kinds of rough usage, to see if they would crack or burst. Surely an engineer should be able to say whether a cylinder is safe without dropping it from the roof or rolling it down the front-door steps to see if it breaks.

These examples refer only to different grades of the same material—steel—but how far worse off we are when the problem is whether some other alloy would be suitable to replace steel. Proposals have been

made, for example, to replace the very hard steel used at present for connecting-rods by duralumin or some other forged aluminium alloy. It seems worth trying; but who, in our present state of ignorance of the real properties of metals, will say if the experiment will be a success?

How difficult it is to prophesy may be illustrated by the results of two empirical tests on duralumin and steel sheets of the same thicknesses. The ultimate strengths and elongations of the steel and the duralumin were roughly equal. The lantern slides show that under reverse-bend tests they both follow the same law, the steel being the better. But under the cupping test they follow opposite laws.

The suitability of different materials presumably depends on their fundamental physical properties. These may be many, but some physicists think that they are probably really very few, and that, knowing these few, it may be possible to deduce all the complex properties required by the engineer and to state with certainty how materials will behave under any conditions of service. This is the most fundamental problem which needs solution to enable the knowledge of the strength of materials to be put on a sound foundation. It will need the co-operation of able physicists, metallurgists, and engineers to solve it.

While urging the importance of research in the fundamental theories of stress and fundamental properties of materials, I wish to lay special stress on the nature of the researches required. Engineers are intensely practical men, and their practice has generally been ahead of their theory. The difficulties they have met have been dealt with, often with the greatest ingenuity and skill, as special problems. They have seldom had time or opportunity to solve the general problems, and as a result they are used to making their experiments and trials as close a copy—usually on a smaller scale—of the real thing as possible. The results obtained in this way, while they are applicable to the particular problem, are of little general use. They depend on many factors. The researches I am now advocating must be of a diametrically opposite description. They must be absolutely general, and the results must depend on one factor only at a time, so that general laws may be established which will be applicable to all special problems.

There are many other similar gaps in our knowledge to which I have not time to refer to-day. I have tried to show that we need most of all a real knowledge of the fundamental properties of materials, from which we shall be able to deduce their behaviour in any condition of service, so that we may be able to compare the relative merits of diverse materials for any particular purpose.

Secondly, that we need a practical method of calculating the stresses in parts of any form, so that concentrations of stress may be avoided or that their magnitudes may be known and allowed for.

Thirdly, that we need a rational connecting link between the tests made on materials and the stresses they will bear in service, to replace the factor of safety. I have suggested two tests, the Proof Load and the Fatigue Limit, which might be used directly in estimating the allowable working stress.



Fourthly, that we need a mathematical theory for the strength of anisotropic materials, of which timber is an extreme and important example.

When the notes for this address were first drafted I ended by an appeal to the Board of Scientific and Industrial Research to undertake the necessary research work. Since then the Aeronautical Research Committee has been constituted, and a sub-committee has been appointed to deal with 'Materials.' I have great hopes that the committee will tackle many of these problems. I will therefore conclude by appealing to all who can help to assist that committee in their endeavour to solve these most important and fascinating, but most difficult, problems.

# British Association for the Advancement of Science.

SECTION H: CARDIFF, 1920.

## ADDRESS TO THE ANTHROPOLOGICAL SECTION

BY

PROF. KARL PEARSON, M.A., LL.D., F.R.S.,

PRESIDENT OF THE SECTION.

*Anthropology*—the Understanding of Man—should be, if Pierre Charron were correct, the true science and the true study of mankind.<sup>1</sup> We might anticipate that in our days—in this era of science—anthropology in its broadest sense would occupy the same exalted position that theology occupied in the Middle Ages. We should hail it ‘Queen of the Sciences,’ the crowning study of the academic curriculum. Why is it that we are Section H and not Section A? If the answer be given that such is the result of historic evolution, can we still be satisfied with the position that anthropology at present takes up in our British Universities and in our learned societies? Have our universities, one and all, anthropological institutes well filled with enthusiastic students, and are there brilliant professors and lecturers teaching them not only to understand man’s past, but to use that knowledge to forward his future? Have we men trained during a long life of study and research to represent our science in the arena, or do we largely trust to dilettanti—to retired civil servants, to untrained travellers or colonial medical men for our knowledge, and to the anatomist, the surgeon, or the archæologist for our teaching? Needless to say, that for the study of man we require the better part of many sciences, we must draw for contributions on medicine, on zoology, on anatomy, on archæology, on folk-lore and travel-lore, nay, on history, psychology, geology, and many other branches of knowledge. But a hotch-potch of the facts of these sciences does not create anthropology. The true anthropologist is not the man who has merely a wide knowledge of the conclusions of other sciences, he is the man who grasps their bearing on mankind and throws light on the past and present factors of human evolution from that knowledge.

<sup>1</sup> “La vraie science et le vrai estude de l’homme c’est l’Homme.” Pierre Charron, *De la Sagesse*, Préface du Premier Livre, 1601. Pope, with his “The proper study of mankind is Man,” 1733, was, as we might anticipate, only a plagiarist.

I am afraid I am a scientific heretic—an outcast from the true orthodox faith—I do not believe in science for its own sake. I believe only in science for man's sake. You will hear on every side the argument that it is not the aim of science to be utile, that you must pursue scientific studies for their own sake and not for the utility of the resulting discoveries. I think that there is a great deal of obscurity about this attitude, I will not say nonsense. I find the strongest supporters of 'science for its own sake' use as the main argument for the pursuit of not immediately utile researches that these researches will be useful some day, that we can never be certain when they will turn out to be of advantage to mankind. Or, again, they will appeal to non-utile branches of science as providing a splendid intellectual training—as if the provision of highly trained minds was not itself a social function of the greatest utility! In other words, the argument from utility is in both cases indirectly applied to justify the study of science for its own sake. In the old days the study of hyperspace—space of higher dimensions than that of which we have physical cognisance—used to be cited as an example of a non-utile scientific research. In view of the facts (i.) that our whole physical outlook on the universe—and with it I will add our whole philosophical and theological outlooks—are taking new aspects under the theory of Einstein; and (ii.) that study of the relative influences of Nature and Nurture in Man can be reduced to the trigonometry of polyhedra in hyperspace—we see how idle it is to fence off any field of scientific investigation as non-utile.

Yet are we to defend the past of anthropology—and, in particular, of anthropometry—as the devotion of our science to an immediate non-utile which one day is going to be utile in a glorious and epoch-making manner, like the Clifford-Einstein suggestion of the curvature of our space? I fear we can take no such flattering unction to our souls. I fear that 'the best is yet to be' cannot be said of our multitudinous observations on 'height-sitting' or on the censuses of eye or hair colours of our population. These things are dead almost from the day of their record. It is not only because the bulk of their recorders were untrained to observe and measure with scientific accuracy, it is not only because the records in nine out of ten cases omit the associated factors without which the record is valueless. It is because the progress of mankind in its present stage depends on characters wholly different from those which have so largely occupied the anthropologist's attention. Seizing the superficial and easy to observe, he has let slip the more subtle and elusive qualities on which progress, on which national fitness for this or that task essentially depends. The pulse-tracing, the reaction-time, the mental age of the men under his control are far more important to the commanding officer—nay, I will add, to the employer of labour—than any record of span, of head-measurement, or pigmentation categories. The psycho-physical and psycho-physiological characters are of far greater weight in the struggle of nations to-day than the superficial measurements of man's body. Physique, in the fullest sense, counts something still, but it is physique as measured by health, not by stature or eye-colour. But character, strength of will, mental quickness count more, and if anthropometry

is to be useful to the State it must turn from these rusty old weapons, these measurements of stature and records of eye-colour to more certain appreciations of bodily health and mental aptitude—to what we may term 'vigorimetry' and to psychometry.

Some of you may be inclined to ask: And how do you know that these superficial size-, shape-, and pigment-characters are not closely associated with measurements of soundness of body and soundness of mind? The answer to this question is twofold, and I must ask you to follow me for a moment into what appears a totally different subject. I refer to a 'pure race.' Some biologists apparently believe they can isolate a pure race, but in the case of man, I feel sure that purity of race is a merely relative term. For a given character one race is purer than a second, if the scientific measure of variation of that character is less than it is in the second. In loose wording, for we cannot express ourselves accurately without mathematical symbols, that race is purer for which on the average the individuals are closer to type for the bulk of ascertainable characters than are the characters in a second race. But an absolutely pure race in man defies definition. The more isolated a group of men has remained, the longer it has lived under the same environment, and the more limited its habitat, the less variation from type it will exhibit, and we can legitimately speak of it as possessing greater purity. We, most of us, probably believe in a single origin of man. But as anthropologists we are inclined to speak as if at the dawn of history there were a number of pure races, each with definite physical and mental characteristics; if this were true, which I do not believe, it could only mean that up to that period there had been extreme isolation, extremely differentiated environments, and so marked differences in the direction and rate of mental and physical evolution. But what we know historically of folk-wanderings, folk-mixings, and folk-absorptions have undoubtedly been going on for hundreds of thousands of years, of which we know only a small historic fragment. Have we any real reason for supposing that 'purity of race' existed up to the beginning of history, and that we have all got badly mixed up since?

Let us, however, grant that there were purer races at the beginning of history than we find to-day. Let us suppose a Nordic race with a certain stature, a given pigmentation, a given shape of head, and a given mentality. And, again, we will suppose an Alpine race, differing markedly in type from the Nordic race. What happens if we cross members of the two races and proceed to a race of hybrids? A Mendelian would tell us that these characters are sorted out like cards from a pack in all sorts of novel combinations. A Nordic mentality will be found with short stature and dark eyes. A tall but brachycephalic individual will combine Alpine mentality with blue eyes. Without accepting fully the Mendelian theory we can at least accept the result of mass observations, which show that the association between superficial physical measurements and mentality is of the slenderest kind. If you keep within one class, my own measurements show me that there is only the slightest relation between intelligence and the size and shape of the head. Pigmentation in this country seems

to have little relation to the incidence of disease. Size and shape of head in man have been taken as a rough measure of size and shape of brain. They cannot tell you more—perhaps not as much as brain-weight—and if brain-weight were closely associated with intelligence, then man should be at his intellectual prime in his teens.

Again, too often is this idea of close association of mentality and physique carried into the analysis of individuals within a human group, *i.e.* of men belonging to one or another of the many races which have gone to build up our population. We talk as if it was our population which was mixed, and not our germplasm. We are accustomed to speak of a typical Englishman. For example, Charles Darwin; we think of his mind as a typical English mind, working in a typical English manner, yet when we come to study his pedigree we seek in vain for 'purity of race.' He is descended in four different lines from Irish kinglets; he is descended in as many lines from Scottish and Pictish kings. He had Manx blood. He claims descent in at least three lines from Alfred the Great, and so links up with Anglo-Saxon blood, but he links up also in several lines with Charlemagne and the Carolingians. He sprang also from the Saxon Emperors of Germany, as well as from Barbarossa and the Hohenstaufens. He had Norwegian blood and much Norman blood. He had descent from the Dukes of Bavaria, of Saxony, of Flanders, the Princes of Savoy, and the Kings of Italy. He had the blood in his veins of Franks, Alamans, Merovingians, Burgundians, and Longobards. He sprang in direct descent from the Hun rulers of Hungary and the Greek Emperors of Constantinople. If I recollect rightly, Ivan the Terrible provides a Russian link. There is probably not one of the races of Europe concerned in the folk-wanderings which has not a share in the ancestry of Charles Darwin. If it has been possible in the case of one Englishman of this kind to show in a considerable number of lines how impure is his race, can we venture to assert that if the like knowledge were possible of attainment, we could expect greater purity of blood in any of his countrymen? What we are able to show may occur by tracing an individual in historic times, have we any valid reason for supposing did not occur in prehistoric times, wherever physical barriers did not isolate a limited section of mankind? If there ever was an association of definite mentality with physical characters, it would break down as soon as race mingled freely with race, as it has done in historic Europe. Isolation or a strong feeling against free inter-breeding—as in a colour differentiation—could alone maintain a close association between physical and mental characters. Europe has never recovered from the general hybridisation of the folk-wanderings, and it is only the cessation of wars of conquest and occupation, the spread of the conception of nationality and the reviving consciousness of race, which is providing the barriers which may eventually lead through isolation to a new linking-up of physical and mental characters.

In a population which consists of non-intermarrying castes, as in India, physique and external appearance may be a measure of the type of mentality. In the highly and recently hybridised nations of Europe there are really but few fragments of 'pure races' left, and it is

hopeless to believe that anthropometric measurements of the body or records of pigmentation are going to help us to a science of the psychophysical characters of man which will be useful to the State. The modern State needs in its citizens vigour of mind and vigour of body, but these are not characters with which the anthropometry of the past has largely busied itself. In a certain sense the school medical officer and the medical officer of health are doing more State service of an anthropological character than the anthropologists themselves.

These doubts have come very forcibly to my notice during the last few years. What were the anthropologists as anthropologists doing during the war? Many of them were busy enough and doing valuable work because they were anatomists, or because they were surgeons, or perhaps even because they were mathematicians. But as anthropologists, what was their position? The whole period of the war produced the most difficult problems in folk-psychology. There were occasions innumerable when thousands of lives and most heavy expenditure of money might have been saved by a greater knowledge of what creates and what damps folk movements in the various races of the world. India, Egypt, Ireland, even our present relations with Italy and America, show only too painfully how difficult we find it to appreciate the psychology of other nations. We shall not surmount these difficulties until anthropologists take a wider view of the material they have to record, and of the task they have before them if they wish to be utile to the State. It is not the physical measurement of native races which is a fundamental feature of anthropometry to-day; it is the psychometry and what I have termed the vigorimetry of white—as well as of dark-skinned men that must become the main subjects of our study.

Some of you may consider that I am overlooking what has been contributed both in this country and elsewhere to the science of folk-psychology. I know at least that Wilhelm Wundt's <sup>2</sup> great work runs to ten volumes. But I also know that in its 5452 pages there is not a single table of numerical measurements, not a single statement of the *quantitative* association between mental racial characters, nor, indeed, any attempt to show numerically the intensity of association between folk-mentality and folk customs and institutions. It is folk-psychology in the same stage of evolution as present-day sociology is in, or as individual psychology was in before the advent of experimental psychology and the correlational calculus. It is purely descriptive and verbal. I am not denying that many sciences must for a long period still remain in this condition, but at the same time I confess myself a firm disciple of Friar Roger Bacon <sup>3</sup> and of Leonardo da Vinci, <sup>4</sup> and believe that we can really know very little

<sup>2</sup> Its last volume also bears evidence of the non-judicial mind of the writer, who expresses strong opinions about recent events in the language of the party historian rather than the man of science.

<sup>3</sup> He who knows not Mathematics cannot know any other science, and what is more cannot discover his own ignorance or find its proper remedies.

<sup>4</sup> Nissuna humana investigatione si po dimandare vera scientia s'essa non passa per le mathematiche dimostrazione.

about a phenomenon until we can actually measure it and express its relations to other phenomena in quantitative form. Now you will doubtless suggest that sections of folk-psychology like Language, Religion, Law, Art—much that forms the substance of cultural anthropology—are incapable of quantitative treatment. I am not convinced that this standpoint is correct. Take only the first of these sections—*Language*. I am by no means certain that there is not a rich harvest to be reaped by the first man who can give unbroken time and study to the statistical analysis of language. Whether he start with roots or with words to investigate the degree of resemblance in languages of the same family, he is likely, before he has done, to learn a great deal about the relative closeness and order of evolution of cognate tongues, whether those tongues be Aryan or Sudanese. And the methods applicable in the case of language will apply in the same manner to cultural habits and ideas. Strange as the notion may seem at first, there is a wide field in cultural anthropology for the use of those same methods which have revolutionised psychometric technique, to say nothing of their influence on osteometry.

The problems of cultural anthropology are subtle, but so indeed are the problems of anthropometry, and no instrument can be too fine if our analysis is to be final. The day is past when the arithmetic of the kindergarten sufficed for the physical anthropologist; the day is coming when mere verbal discussion will prove inadequate for the cultural anthropologist.

I do not say this merely in the controversial spirit. I say it because I want to find a remedy for the present state of affairs. I want to see the full recognition of anthropology as a leading science by the State. I want to see the recognition of anthropology by our manufacturers and commercial men, for it should be at least as important to them as chemistry or physics—the foundations of the Anthropological Institutes with their museums and professors in Hamburg and Frankfurt, have not yet found their parallels in commercial centres here. I want to see a fuller recognition of anthropology in our great scientific societies, both in their choice of members and in the memoirs published. If their doors are being opened to psychology under its new technique, may not anthropology also seek for fuller recognition?

It appears to me that if we are to place anthropology in its true position as the queen of the sciences, we must work shoulder to shoulder and work without intermittence in the following directions: anthropologists must not cease:

(i) To insist that our recorded material shall be such that it is at present or likely in the near future to be utile to the State, using the word 'State' in its amplest sense.

(ii) To insist that there shall be institutes of anthropology, each with a full staff of qualified professors, whose whole energy and time shall be devoted to the teaching of and research in anthropology, ethnology and prehistory. At least three of our chief universities should be provided with such institutes.

(iii) To insist that our technique shall not consist in the mere statement of opinion on the facts observed, but shall follow, if possible

with greater insight, the methods which are coming into use in epidemiology and psychology.

I should like to enlarge a little further on these three insistencies, the fundamental 'planks' of the campaign I have in view.

(i) *Insistence on the Nature of the Material to be dealt with.*

I have already tried to indicate that the problems before us to-day, the grave problems that are pressing on us with regard to the future, cannot be solved by the old material and by the old methods. We have to make anthropology a wise counsellor of the State, and this means a counsellor in political matters, in commercial matters, and in social matters.

The Governments of Europe have had military advisers, financial advisers, transport and food experts in their service, but they have not had ethnological advisers; there have been no highly trained anthropologists at their command. You have only to study the Peace of Versailles to see that it is ethnologically unsound and cannot be permanent. It is no good asking why our well-meaning rulers did not consult our well-meaning anthropologists. I for one confess that we have not in the past dealt with actuality, or if we did deal with actuality, we have not treated it in a manner likely to impress either the executive or the public at large. There lacked far too largely the scientific attitude and the fundamental specialist training. I will not go so far as to say that, if the science of man had been developed to the extent of physical science in all European countries, and had then had its due authority recognised, there would have been no war, but I will venture to say that the war would have been of a different character, and we should not have felt that the fate of European society and of European culture hung in the balance, as at this moment they certainly do.

No one can allow individual inspiration to-day, and you may justly cry a Daniel has no right to issue judgment from the high seat of the feast. Daniel's business is that of the outsider, the stranger, the unwelcome person scrawling on the wall.

Well, if it be hard to learn from friends, let us at least study impassionately from our late foes. Some of my audience may have read the recent manifesto of the German anthropologists, their clarion cry for a new and stronger position of the science of man in academic studies. But the manifesto may have escaped some, and so closely does it fit the state of affairs here that I venture to quote certain portions of it. After reciting the sparsity of chairs for the study of physical and cultural anthropology in the German universities and how little academic weight has been given to such studies, it continues: 'Where these sciences have otherwise found recognition in the universities, they are not represented by specialists, so that anthropology is provided for by the anatomists, ethnology by the geographers, and prehistory by Germanists, archæologists and geologists, and this although, in the present extent of these three sciences, the real command of each one of them demands the complete working powers of an individual. This want



of teaching posts had made itself felt long before the war, so that the number of specialists and of those interested in our science has receded.' <sup>5</sup>

And again:

'During the war we have often experienced how in political pamphlets ethnology and ethnography—even as in the peace treaty of Brest-Litovsk—were used too often as catchwords without their users being clear about the ideas those words convey. The sad results of our foreign policy, the collapse of all our calculations as to national frames of mind, were based in no small degree on ethnographic ignorance; one has only to take for example the case of the Turks. Ethnology should not embrace only the spears and clubs of the savages, but also the psychology and demography of the white races, the European peoples. At this very moment, when the right of self-determination has become a foremost question of the day, the scientific determination of the boundaries of a people and its lands has become a task of the first importance. But our Government of the past knew nothing of the activity of the ethnologists, and the Universities were not in the condition to come to their aid, for ethnological chairs and institutes were wanting. The foundation of such must be the task of the immediate future.' <sup>6</sup>

And once more:

'The problems of the military fitness of our people, of the physical constitution of the various social classes, of the influence of the social and material environment upon them, the problems of the biological grounds for the fall in the birth-rate and its results, of the racial composition of our people, of the eventual racial differences and the accompanying diverse mental capacities of the individual strata, and finally the racial changes which may take place in a folk under the influences of civilisation, and the bearing of all these matters on the fate of a nation, these are problems which can alone be investigated and brought nearer to solution by anthropology. Even now after the war population-problems stand in the forefront of interest, the question of folk-increase and of the falling birth-rate is the vital question of the future.' <sup>7</sup>

I must ask your pardon for quoting so much, but it seems so strongly to point the moral of my tale. If you will study what Germany is feeling and thinking to-day do not hope to find it in the newspaper reports, seek it elsewhere in personal communication or in German writings. Then, I think, you will agree with me that rightly or wrongly there is a conviction spreading in Germany that the war arose and that the war was lost because a nation of professed thinkers had studied all sciences, but had omitted to study aptly the science of man. And in a certain sense that is an absolutely correct conviction, for if the science of man stood where we may hope it will stand in the dim and distant future, man would from the past and the surrounding present have some grasp of future evolution, and so have a greater chance of guiding its controllable factors.

<sup>5</sup> *Correspondenz Blatt*, u.s.w., Jahrg. L. S. 37.

<sup>6</sup> *Ibid.* S. 41.

<sup>7</sup> *Ibid.* S. 38

We are far indeed from that to-day; but it befits us none the less to study what this new anthropological movement in Germany connotes. It means that the material of anthropology is going to change, or rather that its observations will be extended into a study of the mental as well as the physical characters, and these of the white races as well as of the dark. It means that anthropologists will not only study individual psychology, but folk-psychology. It means—and this is directly said—that Germany, having lost her colonies, will still maintain her trade by aid of consuls, missionaries, traders, travellers, and others trained academically to understand both savage and civilised peoples. This is a perfectly fair field, and if the game be played squarely can solely lead to increased human sympathy, and we shall only have ourselves to blame if other nations are before us in their anthropological knowledge and its practical applications. The first condition for State support is that we show our science to be utile by turning to the problems of racial efficiency, of race-psychology, and to all those tasks that Galton described as the first duty of a nation—‘in short, to make every individual efficient both through Nature and by Nurture’.

Does this mean that we are to turn our backs on the past, to desert all our prehistoric studies and to make anthropology the servant of sanitation and commerce? Not in the least; if you think this is my doctrine I have indeed failed to make myself even roughly clear to-day. Such teaching is wholly opposed to my view of the function of science. I feel quite convinced that you cannot understand man of to-day, savage or civilised, his body or his mind, unless you know his past evolution, unless you have studied fully all the scanty evidence we have of the stages of his ascent. I should like to illustrate this by an incident which came recently to my notice, because it may indicate to some of those present the difficulties with which the anthropologist has to contend to avoid misunderstanding.

Looking into the ancestry of man and tracing him backward to a being who was not man and was not ape, had this prot-simio-human, in the light of our present knowledge, more resemblance to the gibbon or to the chimpanzee as we know them to-day? Some naturalists link man up to the apes by a gibbonlike form, others by a troglodyte type of ancestor. Some would make a push to do without either. But granted the alternative, which is the more probable? This is the problem of the hylobatic or the troglodyte origin of man. I had given a lecture on the subject, confining my arguments solely to characters of the thigh-bone. Now there chanced to be a statesman present, a man who has had large responsibilities in the government of many races. I have been honoured by seeing his comments on my lecture. ‘I am not,’ he says, ‘particularly interested in the descent of man. I do not believe much in heredity, and this scientific pursuit of the dead bones of the past does not seem to me a very useful way of spending life. I am accustomed to this mode of study; learned volumes have been written in Sanscrit to explain the conjunction of the two vowels “a” and “u.” It is very learned, very ingenious, but not very helpful. . . . I am not concerned with my genealogy so much as with my future. Our intellects can be more advantageously employed than in

finding our diversity from the ape . . . There may be no spirit, no soul: there is no proof of their existence. If that is so, let us do away with shams and live like animals. If, on the other hand, there is a soul to be looked after, let us all strain our nerves to the task; there is no use in digging into the sands of time for the skeletons of the past: build your man for the future.'

What is the reply of anthropology to this indictment of the statesman? You cannot brush it lightly aside. It is the statement of a good man and a strong man who is willing to spend his life in the service of his fellows. He sees us handling fossils and potsherds and cannot perceive the social utility of our studies. He does not believe any enthusiasm for human progress can lie beneath the spade and callipers of the scientific investigator. He has never grasped that the man of to-day is precisely what heredity and his genealogy, his past history and his prehistory, have made him. He does not recognise that it is impossible to build your man for the future until you have studied the origin of his physical and mental constitution. Whence did he draw his good and evil characteristics—are they the product of his nature or his nurture? Man has not a plastic mind and body which the enthusiastic reformer can at will mould to the model of his golden age ideals. He has taken thousands of years to grow into what he is, and only by like processes of evolution—intensified and speeded up, if we work consciously and with full knowledge of the past—can we build his future.

It *does* matter in regard to the gravest problems before mankind to-day whether our ancestry was hylobatic or troglodyte. For five years the whole world has been a stage for brutality and violence. We have seen a large part of the youth who were best fitted mentally and physically to be parents of future generations perish throughout Europe: the dysgenic effect of this slaughter will show itself each twenty to twenty-five years for centuries to come in the census returns of half the countries of the world. Science undertook work which national feeling bid it do, but on which it will ever look back with a shuddering feeling of distaste, an uneasy consciousness of having soiled its hands. And as aftermath we see in almost every land an orgy of violent crime, a sense of lost security, and at times we dread that our very civilisation may perish owing to the weakening of the social ties, a deadening of the responsibilities of class to class. This outbreak of violence which has so appalled the thinking world, is it the sporadic appearance of an innate passion for the raw and brutal in mankind, or is it the outcome of economic causes forcing the nations of the world to the combat for limited food and material supplies? I wish we could attribute it to the latter source, for then we could eradicate the spirit of violence by changing environmental conditions. But if the spirit of violence be innate in man, if there be times when he not only sees red but rejoices in it—and that was the strong impression I formed when I crossed Germany on August 1, 1914—then outbreaks of violence will not cease till troglodyte mentality is bred out of man. That is why the question of troglodyte or hylobatic ancestry is not a pursuit of dead bones. It is a vital problem on which turns much of folk-psychology. It is a

problem utile to the State, in that it throws light on whether nature or nurture is the more likely to build up man's future—and save him from the recurrence of such another quinquennium.

The critic to whom I have referred was not idle in his criticism. He had not been taught that evolutionary doctrine has its bearings on practical life. The biologist and the anthropologist are at fault; they have too often omitted to show that their problems have a very close relation to those of the statesman and the social reformer, and that the problems of the latter cannot be solved without a true insight into man's past, without a knowledge of the laws of heredity, and without a due appreciation of the causes which underlie great folk-movements.

(ii) *Insistence on Institutes of Anthropology.*

The anthropological problems of the present day are so numerous and so pressing that we can afford to select those of the greatest utility. Indeed, the three university institutes of anthropology I have suggested would have to specialise and then work hard to keep abreast of the problems which will crowd upon them. One might take the European races, another Asia and the Pacific, and a third Africa. America in anthropology can well look after itself. In each case we need something on the scale of the Paris Ecole d'Anthropologie, with its seventeen professors and teachers, with its museums and journals. But we want something else—a new conception of the range of problems to be dealt with and a new technique. From such schools would pass out men with academic training fit to become officials, diplomatic agents, teachers, missionaries, and traders in Europe, in Asia, or in Africa, men with intelligent appreciation of and sympathy with the races among whom they proposed to work.

But this extra-State work, important as it is, is hardly comparable in magnitude with the intra-State work which lies ready to hand for the anthropological laboratory that has the will, the staff, and the equipment to take it up efficiently. In the present condition of affairs it is only too likely that much of this work, being psychometric, will fall into the hands of the psychologist, whereas it is essentially the fitting work of the anthropologist, who should come to the task, if fitly trained, with a knowledge of comparative material and of the past history, mental and physical, of mankind, on which his present faculties so largely depend. The danger has arisen because the anthropometer has forgotten that it is as much his duty to measure the human mind as it is his duty to measure the human body, and that it is as much his duty to measure the functional activities of the human body—its dynamical characters—as its statical characters. By dynamical characters I understand such qualities as resistance to fatigue, facility in physical and mental tasks, immunity to disease, excitability under stimuli, and many kindred properties. If you tell me that we are here trenching on the field of psychology and medicine, I reply: Certainly; you do not suppose that any form of investigation which deals with man—body or mind—is to be omitted from the science of man? If you do you have failed to grasp why anthropology is the queen of the sciences. The University anthropological institute of the future will

have attached to it a psychologist, a medical officer, and a biologist. They are essential portions of its requisite staff, but this is a very different matter from lopping off large and important branches of its fitting studies, to be neglected on the ground, or to be dragged away, as dead wood, to be hewn and shapen for other purposes by scientific colleagues in other institutes. Remember that I am emphasising that side of anthropology which studies man in the service of the State—anthropology as a utile science—and that this is the only ground on which anthropology can appeal for support and sympathy from State, from municipality, and from private donors. You will notice that I lay stress on the association of the anthropological institute with the university, and the reasons for this are manifold. In the first place, every science is stimulated by contact with the workers in allied sciences; in the second place, the institute must be a teaching as well as a researching body, and it can only do this effectively in association with an academic centre—a centre from which to draw its students and to recruit its staff. In the third place, a great university provides a wide field for anthropometric studies in its students and its staff. And the advantages are mutual. It is not of much service to hand a student a card containing his stature, his weight, his eye colour, and his head length! Most of these he can find out for himself! But it is of importance to him to know something of how his eye, heart, and respiration function; it is of importance to him to know the general character of his mental qualities, and how they are associated with the rapidity and steadiness of muscular responses. Knowledge on these points may lead him to a fit choice of a career, or at any rate save him from a thoroughly bad choice.

In the course of my life I have often received inquiries from schoolmasters of the following kind: We are setting up a school anthropometric laboratory, and we propose to measure stature, weight, height sitting, &c. Can you suggest anything else we should measure?

My invariable reply is: Don't start measuring anything at all until you have settled the problems you wish to answer, and then just measure the characters in an adequate number of your boys, which will enable you to solve those problems. Use your school as a laboratory, not as a weighhouse.

And I might add, if I were not in dread of giving offence: And most certainly do not measure anything at all if you have *no* problem to solve, for unless you have you cannot have the true spirit of the anthropologist, and you will merely increase that material up and down in the schools of the country which nobody is turning to any real use.

Which of us, who is a parent, has not felt the grave responsibility of advising a child on the choice of a profession? We have before us, perhaps, a few meagre examination results, an indefinite knowledge of the self-chosen occupations of the child, and perhaps some regard to the past experience of the family or clan. Possibly we say John is good with his hands and does not care for lessons; therefore he should be an engineer. That may be a correct judgment if we understand by engineer, the engine-driver or mechanic. It is not true if we think of the builders of Forth Bridges and Assuan Dams. Such men work

with the head and not the hand. One of the functions of the anthropological laboratory of a great university, one of the functions of a school anthropometric laboratory, should be to measure those physical and mental characters and their inter-relations upon which a man's success in a given career so much depends. Its function should be to guide youth in the choice of a calling, and in the case of a school to enable the headmaster to know something of the real nature of individual boys, so that that much-tried man does not feel compelled to hide his ignorance by cabalistic utterances when parents question him on what their son is fitted for.

Wide, however, as is the anthropometric material in our universities and public schools, it touches only a section of the population. The modern anthropologist has to go further; he has to enter the doors of the primary schools; he has to study the general population in all its castes, its craftsmen, and its sedentary workers. Anthropology has to be useful to commerce and to the State, not only in association with foreign races, but still more in the selection of the right men and women for the staff of factory, mine, office, and transport. The selection of workmen to-day by what is too often a rough trial and discharge method is one of the wasteful factors of production. Few employers even ask what trades parents and grandparents have followed, nor consider the relation of a man's physique and mentality to his proposed employment. I admit that progress in this direction will be slow, but if the work undertaken in this sense by the anthropologist be well devised, accurate, and comprehensive, the anthropometric laboratory will gradually obtain an assured position in commercial appreciation. As a beginning, the anthropologist by an attractive museum, by popular lectures and demonstrations, should endeavour to create, as Sir Francis Galton did at South Kensington, an anthropometric laboratory frequented by the general population, as well as by the academic class. Thus he will obtain a wider range of material. But the anthropologist, if he is to advance his science and emphasise its services to the State, must pass beyond the university, the school, and the factory. He must study what makes for wastage in our present loosely organised society; he must investigate the material provided by reformatory, prison, asylums for the insane and mentally defective; he must carry his researches into the inebriate home, the sanatorium, and the hospital, side by side with his medical collaborator. Here is endless work for the immediate future, and work in which we are already leagues behind our American colleagues. For them the psychometric and anthropometric laboratory attached to asylum, prison, and reformatory is no startling innovation, to be spoken of with bated breath. It is a recognised institution of the United States to-day, and from such laboratories the 'fieldworkers' pass out, finding out and reporting on the share parentage and environment have had in the production of the abnormal and the diseased, of the anti-social of all kinds. Some of this work is excellent, some indifferent, some perhaps worthless, but this will always be the case in the expansion of new branches of applied science. The training of the workers must be largely of an experimental character, the technique has to be devised as the work develops. Instructors and directors have to be

appointed, who have not been trained *ad hoc*. But this is remedying itself, and if indeed, when we start, we also do not at first limp somewhat lamely along these very paths, it will only be because we have the advantage of American experience.

There is little wonder that in America anthropology is no longer the stepchild of the State. It has demanded its heritage, and shown that it can use it for the public good.

If I have returned to my first insistence that the problems handled by the anthropologist shall be those useful to the State, it is because I have not seen that point insisted upon in this country, and it is because my first insistence, like my third, involves the second for its effectiveness—the establishment in our chief universities of anthropological institutes. As Gustav Schwalbe said of anthropology in 1907—and he was a man who thought before he spoke, and whose death during the war is a loss to anthropologists the whole world over—‘a lasting improvement can only arise if the State recognises that anthropology is a science pre-eminently of value to the State, a science which not only deserves but can demand that chairs shall be officially established for it in every university. . . Only this spread of officially authorised anthropology in all German universities can enable it to fulfil its task, that of training men who, well armed with the weapon of anthropological knowledge, will be able to place their skill at the service of the State, which will ever have need of them in increasing numbers.’<sup>s</sup>

Our universities are not, as in Germany, Government controlled institutions, although such control is yearly increasing. Here we have first to show that we are supporting the State before the State somewhat grudgingly will give its support to us. Hence the immediate aim of the anthropologist should be—not to suggest that the State should *a priori* assist work not yet undertaken, but to do what he can with the limited resources in his power, and when he has shown that what he has achieved is, notwithstanding his limitations, of value to the State, then he is in a position to claim effective support for his science.

I have left myself little time to place fairly before you my third insistence.

### (iii) *Insistence on the Adoption of a New Technique.*

What is it that a young man seeks when he enters the university— if we put aside for a moment any social advantages, such as the formation of lifelong friendships associated therewith? He seeks, or ought to seek, training for the mind. He seeks, or ought to seek, an open doorway to a calling which will be of use to himself, and wherein he will take his part, a useful part, in the social organisation of which he finds himself a member. Much as we may all desire it, in the pressure of modern life, it is very difficult for the young man of moderate means to look upon the university training as something apart from his professional training. Men more and more select their academic studies with a view to their professional value. We can no longer combine the senior wranglership with the pursuit of a judgeship; we cannot

<sup>s</sup> *Correspondenz Blatt*, Jahrg. xxxviii. S. 68.

pass out in the classical tripos and aim at settling down in life as a Harley Street consultant; we cannot take a D.Sc. in chemistry as a preliminary to a journalistic career. It is the faculties which provide professional training that are crowded, and men study nowadays physics or chemistry because they wish to be physicists or chemists, or seek by their knowledge of these sciences to reach commercial posts. Even the very Faculty of Arts runs the danger of becoming a professional school for elementary school teachers. I do not approve this state of affairs: I would merely note its existence. But granted it, what does anthropology offer to the young man who for a moment considers it as a possible academic study? There are no professional posts at present open to him, and few academic posts.<sup>9</sup> There is little to attract the young man to anthropology as a career. Is its position as a training of mind any stronger? The student knows if he studies physics or chemistry or engineering that he will obtain a knowledge of the principles of observation, of measurement, and of the interpretation of data, which will serve him in good stead whenever he has to deal with phenomena of any kind. But, alas! in anthropology, while he finds many things of surpassing interest, he discovers no generally accepted methods of attacking new problems, *quot homines, tot sententiæ*. The type of man we want in anthropology is precisely the man who now turns to mathematics, to physics, and to astronomy—the man with an exact mind who will not take statements on authority and who believes in testing all things. To such a man anthropometry—in all its branches, craniometry, psychometry, and the wide field in which body and mind are tested together under dynamic conditions—forms a splendid training, *provided* his data and observations are treated as seriously as those of the physicist or astronomer by adequate mathematical analysis. Such a type of man is at once repelled from our science if he finds in its text-books and journals nothing but what has been fitly termed ‘kindergarten arithmetic.’ Why the other day I saw in a paper by a distinguished anthropologist an attempt to analyse how many individual bones he ought to measure. He adopted the simple process of comparing the results he obtained when he took 10, 20, 30 individuals. He was not really wiser at the end of his analysis than at the beginning, though he thought he was. And this, notwithstanding that the whole matter had been thrashed out scientifically by John Bernoulli two centuries ago, and that its solution is a commonplace of physicist and astronomer!

How can we expect the scientific world to take us seriously and to treat anthropology as the equal of other sciences while this state of affairs is possible? What discipline in logical exactness are we offering to academic youth which will compare with that of the older sciences? What claim have we to advise the State until we have introduced a sounder technique and ceased to believe that anthropometry is a science that any man can follow, with or without training? As I have hinted, the problems of anthropology seem to me as subtle as

<sup>9</sup> In London, for example, there is a reader in physical anthropology who is a teacher in anatomy, and a professorship in ethnology, which for some mysterious reason is included in the faculty of economics and is, I believe, not a full-time appointment.



those of physical astronomy, and we are not going to solve them with rusty weapons, nor solve them at all unless we can persuade the 'brammy boys' of our universities that they are worthy of keen minds. Hence it seems to me that the most fertile training for academic purposes in anthropology is that which starts from anthropometry in its broadest sense, which begins to differentiate caste and class and race, bodily and mental health and disease, by measurement and by the analysis of measurement. Once this sound grounding has been reached the trained mind may advance to ethnology and sociology, to prehistory and the evolution of man. And I shall be surprised if equal accuracy of statement and equal logic of deduction be not then demanded in these fields, and I am more than half convinced, nay, I am certain, that the technique the student will apply in anthropometry can be equally well applied in the wider fields into which he will advance in his later studies. Give anthropology a technique as accurate as that of physics, and it will forge ahead as physics have done, and then anthropologists will take their due place in the world of science and in the service of the State.

Francis Galton has a claim upon the attention of anthropologists which I have not. He has been President of your Institute, and he spoke just thirty-five years ago from the chair I now occupy, pressing on you for the first time the claims of new anthropological methods. In Galton's words: 'Until the phenomena of any branch of knowledge have been submitted to measurement and number it cannot assume the status and dignity of a Science' Have we not rather forgotten those warning words, and do they not to some extent explain why our universities and learned societies, why the State and statesmen, have turned the cold shoulder on anthropology?

This condition of affairs must not continue; it is good neither for anthropology, nor for the universities, nor for the State if this fundamental science, the science of man, remains in neglect. It will not continue if anthropologists pull together and insist that their problems shall not fail in utility, that their scientific technique shall be up to date, and that anthropological training shall be a reality in our universities—that these shall be fully equipped with museums, with material, with teachers and students.

It is almost as difficult to reform a science as it is to reform a religion; in both cases the would-be reformer will offend the sacrosanct upholders of tradition, who find it hard to discard the faith in which they have been reared. But it seems to me that the difficulties of our time plead loudly for a broadening of the purpose and a sharpening of the weapons of anthropology. If we elect to stand where we have done a new science will respond to the needs of State and society; it will spring from medicine and psychology, it will be the poorer in that it knows little of man's development, little of his history or prehistory. But it will devote itself to the urgent problems of the day. The future lies with the nation that most truly plans for the future, that studies most accurately the factors which will improve the racial qualities of future generations either physically or mentally. Is anthropology to lie outside this essential function of the science of

man? If I understand the recent manifesto of the German anthropologists, they are determined it shall not be so. The war is at an end, but the critical time will be with us again, I sadly fear, in twenty to thirty years. How will the States of Europe stand then? It depends to no little extent on how each of them may have cultivated the science of man and applied its teaching to the improvement of national physique and mentality. Let us take care that our nation is not the last in this legitimate rivalry. The organisation of existing human society with a view to its future welfare is the crowning task of the science of man; it needs the keenest-minded investigators, the most stringent technique, and the utmost sympathy from all classes of society itself. Have we, as anthropologists, the courage to face this greatest of all tasks in the light of our knowledge of the past and with our understanding of the folk of to-day? Or shall we assert that anthropology is after all only a small part of the science of man, and retreat to our study of bones and potsherds on the ground that science is to be studied for its own sake and not for the sake of mankind? I do not know what answer you will give to that question, yet I am convinced what the judgment of the future on your answer is certain to be.



# British Association for the Advancement of Science.

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SECTION I: CARDIFF, 1920.

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## ADDRESS TO THE PHYSIOLOGICAL SECTION

BY

J. BARCROFT, C.B.E., M.A., F.R.S.,

PRESIDENT OF THE SECTION.

PROMINENT among the pathological conditions which claimed attention during the war was that of insufficient oxygen supply to the tissues, or anoxæmia. For this there were several reasons; on the one hand, anoxæmia clearly was a factor to be considered in the elucidation of such conditions as are induced by gas-poisoning, shock, &c. On the other hand, knowledge had just reached the point at which it was possible to discuss anoxæmia on a new level. It is not my object in the present address to give any account of war-physiology—the war has passed, and I, for one, have no wish to revive its memories, but anoxæmia remains, and, as it is a factor scarcely less important in peace than in war pathology, I think I shall not do wrong in devoting an hour to its consideration.

The object of my address, therefore, will be to inquire, and, if possible to state, where we stand; to sift, if I can, the knowledge from the half-knowledge; to separate what is ascertained as the result of unimpeachable experiment from what is but guessed on the most likely hypothesis. In war it was often necessary to act on defective information, because action was necessary and defective information was the best that was to be had. In this, as in many other fields of knowledge, the whole subject should be reviewed, the hypotheses tested experimentally, and the gaps filled in. The sentence which lives in my mind as embodying the problem of anoxæmia comes from the pen of one who has given more concentrated thought to the subject than perhaps any other worker—Dr. J. S. Haldane.<sup>1</sup> It runs, 'Anoxæmia not only stops the machine but wrecks the machinery.' This phrase puts the matter so clearly that I shall commence by an inquiry as to the limits within which it is true.

Anything like complete anoxæmia stops the machine with almost incredible rapidity. It is true that the breath can be held for a considerable time, but it must be borne in mind that the lungs have a volume of about three litres at any moment, that they normally contain about half a litre of oxygen, and that this will suffice for the body at rest for upwards of two minutes. But get rid of the residual oxygen from the lungs only to the very imperfect extent which is pos-

<sup>1</sup> See References, page 17.

sible by the breathing of some neutral gas, such as nitrogen, and you will find that only with difficulty will you endure half a minute. Yet even such a test gives no real picture of the impotence of the machine—which is the brain—to ‘carry on’ in the absence of oxygen. For, on the one hand, nearly a quarter of a minute elapses before the reduced blood gets to the capillary in the brain, so that the machine has only carried on for the remaining quarter of a minute; on the other hand, the arterial blood under such circumstances is far from being completely reduced—in fact, it has very much the composition of ordinary venous blood, which means that it contains about half its usual quatum of oxygen. It seems doubtful whether complete absence of oxygen would not bring the brain to an instantaneous standstill. So convincing are the experimental facts to anyone who has tested them for himself, that I will not further labour the power of anoxæmia to stop the machine. I will, however, say a word about the assumption which I have made that the machine in this connection is the brain.

It cannot be stated too clearly that anoxæmia in the last resort must affect every organ of the body directly. Stoppage of the oxygen supply is known, for instance, to bring the perfused heart to a standstill, to cause a cessation of the flow of urine, to produce muscular fatigue, and at last immobility, but from our present standpoint these effects of anoxæmia seem to me to be out of the picture, because the brain is so much the most sensitive to oxygen want. Therefore, if the problem is the stoppage of the machine due to an insufficient general supply of oxygen, I have little doubt that the machine stops because the brain stops, and here at once I am faced with the question how far is this assumption and how far is it proven fact? I freely answer that research in this field is urgent; at present there is too great an element of assumption, but there is also a certain amount of fact.

To what extent does acute anoxæmia in a healthy subject wreck the machinery as well as stop the machine? By acute anoxæmia I mean complete or almost complete deprivation of oxygen which, in the matter of time, is too short to prove fatal. It is not easy to obtain quite clear-cut experiments in answer to the above question. No doubt many data might be quoted of men who have recovered from drowning, &c. Such data are complicated by the fact that anoxæmia has only been a factor in their condition; other factors, such as accumulation of carbonic acid, may also have contributed to it. The remarkable fact about most of them, however, is the slightness of the injury which the machine has suffered. These data, therefore, have a value in so far as they show that a very great degree of anoxæmia, if acute and of short duration, may be experienced with but little wreckage to the machine. They have but little value in showing that such wreckage is due to the anoxæmia, because the anoxæmia has not been the sole disturbance. Of such cases I will quote two.

The first is that of a pupil of my own who received a gunshot wound in the head, to the prejudice of his cerebral circulation. I can give you the most perfect assurance that neither intellectually nor physically has the catastrophe which befell him caused any permanent injury. For the notes of the case I am indebted to

Colonel Sir Willham Hale White. My pupil fell wounded at 6.50 A.M. on 20th of November 1917. As far as is known, he lay insensible for about two hours. Picked up five hours after the wound, he could not move either upper or lower extremity. Thirty-six hours after the wound he underwent an operation which showed that the superior sagittal sinus was blocked, happily not by a thrombus, but by being torn and having pieces of the inner table of the skull thrust into it, so that for this period of time the motor areas on both sides down to the face area were asphyxiated, the right being much more affected than the left. Six days after the wound the cerebation was still slow, with typically vacant look.

In the left upper extremity the muscular power was much improved; he could raise his arm to his mouth, but he preferred to be fed.

In the right upper extremity there were no movements in the shoulder, elbow, or wrist, but he could flex and extend the fingers weakly.

In the left lower extremity there were fair movements of hip and knee, no movements of ankle and toes.

In the right lower extremity there were no movements of hip, knee, ankle, or toes.

Six weeks after the wound he first walked, although with difficulty; absolutely the last place in which the paresis remained was in the muscles of the right toes. Four months after the injury these toes were spread out and could not be brought together voluntarily. Gradually this has become less, but even now, two and a-half years later, all these muscles are weaker than those on the other side.

Such, then, is the wreckage of the machine caused by thirty-six hours' blockage of the blood-flow through the motor areas of the brain; wreckage enough but not irreparable.

I pass to the second case. It is that of the child of a well-known Professor of Physiology and a first authority on the subject of respiration. I am indebted to him for the following notes:

In this child, a male twin, born about twelve weeks too early, it was noticed about twenty-four hours after birth that the breathing during sleep was irregular and of a very pronounced Cheyne Stokes type, six to ten breaths being followed by a pause during which no respiratory movement occurred. Usually the pauses were of fifteen to twenty seconds' duration, but occasionally (two to ten times a day) the breathing remained suspended for a prolonged period, extending in some cases to ten and fifteen minutes, and in one accurately observed case even to twenty-five minutes, interrupted by one single breath and a cry about the middle of the period. The breathing invariably started again before the heart-beats ceased.

Cases in which the anoxæmia has been uncomplicated are to be found among those who have been exposed to low atmospheric pressures; for instance, balloonists and aviators. Of these quite a considerable number have suffered from oxygen want to the extent of being unconscious for short intervals of time.

No scientific observer has pushed a general condition of anoxæmia either on himself or on his fellows to the extent of complete unconscious-

ness. The most severe experiments of this nature are those carried out by Haldane and his colleagues. One experiment in particular demands attention. Dr. Haldane and Dr. Kellas<sup>2</sup> together spent an hour in a chamber in which the air was reduced to between 320 and 295 mm. It is difficult to say how far they were conscious. Clearly each believed the other to be complete master of his own faculties, but it is evident that Dr. Haldane was not so. I gather that he has no recollection of what took place, that whenever he was consulted about the pressure he gave a stereotyped answer which was the same for all questions, that, even with a little more oxygen present, he was sufficiently himself to wish to investigate the colour of his lips in the glass, but insufficiently himself to be conscious that he was looking into the back and not the front of the mirror. Dr. Kellas, who could make observations, never discovered Dr. Haldane's mental condition, though boxed up with him for an hour, and went on consulting him automatically. A somewhat similar experiment was performed on the other two observers, with results differing only in degree.

Yet the after-effects are summed up in the following sentence: 'All four observers suffered somewhat from headache for several hours after these experiments, but there was no nausea or loss of appetite.'

Of real importance in this connection are the results of carbon monoxide poisoning. Of these a large number might be cited. Those interested will find some very instructive cases described in a volume entitled 'The Investigation of Mine Air,' by the late Sir C. Le Neve Foster and Dr. Haldane.<sup>3</sup> The cases in question were those of a number of officials who went to investigate the mine disaster on Snaefell, in the Isle of Man, in May 1897. Of the five cases cited all suffered some after-effects, by which I mean that by the time the blood was restored sufficiently to its normal condition for the tissues to get the amount of oxygen which they required, the effects of the asphyxia had not passed off and to this extent the machine suffered, *e.g.*

Mr. W. H. Kitto says: 'On reaching *terra firma* I felt very ill and wanted to vomit . . . through the night. I had severe palpitation of the heart, a thing I have never felt in my life before or since. On the day following, Thursday, the pain in my knees was so great that I could not stand properly, and for fully a week I had great pain when walking, and still (a month later) feel slight effects of the poisoning.'

Of the five whose experiences were given, the one who received the most permanent damage was Sir Clement Foster himself.

A few days after I got back from the island the first time, about the 21st or 22nd of May, I noticed my heart; it could scarcely be called palpitation, as I understand palpitations to be, for there did not seem to be any increased rapidity of the action, but I was conscious of its beating; as a rule, I am not. This passed off, and then on the 1st and 2nd of June I noticed it very decidedly again, so much so that I went to my doctor. He sounded me, and said the heart was all right, though there was one sound which was not very distinct. This consciousness of having a heart still returns from time to time, though only to a slight extent. On the 19th May I suffered much from headache, not regularly, but intermittently. The headache lasted for several days, and the feeling in the legs was very apparent; it was an aching in the legs from the knees to the ankles. A coldness from the knees to the soles of the feet was also noticeable; it came on occasionally for a considerable time. The head-

aches continued at intervals for some time, and lasted certainly for some months after the accident; indeed, I cannot say that they have disappeared altogether.

To sum up, then, what may be said of the permanent damage caused by acute anoxæmia, it seems to me to be as follows: No degree of anoxæmia which produces a less effect than that of complete unconsciousness leaves anything more than the most transient effects; if the anoxæmia be pushed to the point at which the subject is within a measurable distance of death, the results may take days or weeks to get over, but only in the case of elderly or unsound persons is the machine wrecked beyond repair.

#### *Chronic Anoxæmia.*

And now to pass to the consideration of what I may call chronic anoxæmia; that is to say, oxygen want which perhaps is not very great in amount—and I shall have to say something later about the measurement of anoxæmia in units—but which is continued over a long time: weeks, months, or perhaps years. We have to ask ourselves, how far does chronic anoxæmia stop the machinery; how far does it wreck the machine? Here we are faced with a much more difficult problem, for the distinction between stopping the machinery and wrecking the machine tends to disappear. In fact so indistinct does it become that you may ask yourself, with some degree of justice, does chronic anoxæmia stop the machine in any other way than by wrecking it?

The most obvious instances of men subject to chronic anoxæmia are the dwellers at high altitudes. Now, it is quite clear that in many instances of mountain-dwellers the anoxæmia does not wreck the machine. On what I may call the average healthy man anoxæmia begins to tell at about 18,000 feet. At lower altitudes no doubt he will have some passing trouble, but it seems to me from my own experience that this altitude is a very critical one. Yet there are mining camps at such heights in South America at which the work of life is carried on. Under such circumstances the machine is kept going by a process of compensation. This is in part carried out by a modification in the chemical properties of the blood. It would appear that both the carbonic acid in the blood and the alkali diminish. The result, according to my interpretation of my own observations on the Peak of Teneriffe, which appeared to be confirmed by the experiments in a chamber in Copenhagen<sup>4</sup> from which the air was partially exhausted, was this: The hydrogen ion concentration of the blood increased slightly, the respiratory centre worked more actively, and the lung became better ventilated with oxygen, with the natural result that the blood became more oxygenated than it would otherwise have been.

The difference which this degree of acclimatisation made was very great. Take as an example one of my colleagues, Dr. Roberts. On Monte Rosa in his case, as in that of the rest of the party, 15 mm. of oxygen pressure were gained in the lungs. To put the matter another way, the amount of oxygen in our lungs at the summit was what it would otherwise have been 5,000 or 6,000 feet lower down. No actual analyses of the oxygen in Roberts' arterial blood were made, but from what we now know it seems probable that his blood was about 82 per



cent. saturated; that is to say that for every hundred grams of hæmoglobin in the blood 82 were oxyhæmoglobin and 18 reduced hæmoglobin; had this degree of acclimatisation not taken place his blood would have contained as many as 38 parts of unoxidised hæmoglobin out of every hundred, and this would probably have made all the difference between the machine stopping and going on.

The body, then, had fought the anoxæmia and reduced it very much in degree, but at the same time the anoxæmia had, in a subtle way, done much to stop the powers of the body, for this very acclimatisation is effected at the expense of the ultimate reserve which the body has at its disposal for the purpose of carrying out muscular or other work. The oxygen in the lungs was obtained essentially by breathing at rest as you would normally do when taking some exercise. Clearly, then, if you are partly out of breath before you commence exercise you cannot undertake so much as you otherwise would do. As a friend of mine, who has, I believe, camped at a higher altitude than any other man, put it to me, 'So great was the effort that we thought twice before we turned over in bed.'

One of the interesting problems with regard to chronic anoxæmia is its effect upon the mind. Working from the more acute type of anoxæmia to the more chronic, the following quotation will give an account of the condition of a person in the acute stage. It is Sir Clement Le Neve Foster's account of himself during CO poisoning, and shows loss of memory, some degree of intelligence, and a tendency to repeat what is said:

How soon I realised that we were in what is commonly called 'a tight place' I cannot say, but eventually, from long force of habit, I presume, I took out my note-book. At what o'clock I first began to write I do not know, for the few words written on the first page have no hour put to them. They were simply a few words of good-bye to my family, badly scribbled. The next page is headed '2 P.M.,' and I perfectly well recollect taking out my watch from time to time. As a rule I do not take a watch underground, but I carried it on this occasion in order to be sure that I left the rat long enough when testing with it. In fact, my note on the day of our misadventure was '5th ladder. Rat two minutes at man,' meaning by the side of the corpse. My notes at 2 P.M. were as follows: '2 P.M., good-bye, we are all dying, your Clement, I feel we are dying, good-bye, all my darlings all, no help coming, good-bye we are dying, good-bye, good-bye we are dying, no help comes, good-bye, good-bye.' Then later, partly scribbled over some 'good-byes' I find, 'We saw a body at 1.30 and then all became affected by the bad air, we have got to the 115 and can get no further, the box does not come in spite of our ringing for help. It does not come, does not come—I wish the box would come. Captain R. is shouting, my legs are bad, and I feel very <sup>1</sup>, my knees are <sup>1</sup>.' The so-called 'ringing' was signalling to the surface by striking the air-pipe with a hammer or bar of iron. We had agreed upon signals before we went down. There is writing over other writing, as if I did not see exactly where I placed my pencil, and then: 'I feel as if I were dreaming, no real pain, good-bye, good-bye, I feel as if I were sleeping.' '2.15 we are all done. No <sup>1</sup>, or scarcely any, we are done, we are done, good-bye my darlings.' Here it is rather interesting to note the 'godo' instead of 'good.' Before very long the fresh men who had climbed down to rescue us seemed to have arrived, and explained that the 'box' was caught in the shaft. Judging by my notes I did not realise thoroughly that we should be rescued. Among them occur the words 'No pain, it is merely like a dream, no pain.' I frequently wrote the same sentence over and over again. My last note on reaching the surface tells of

<sup>1</sup> in the above quotation indicates an illegible word in the notes.

that resistance to authority which likewise appears to be a symptom of the poisoning.

These notes afford ample confirmation of the effect produced by carbonic oxide poisoning of causing reiteration. I wrote the same words over and over again unnecessarily. The condition I was in was rather curious. I had absorbed enough of the poison to paralyse me to a certain extent and dull my feelings, but at the same time my reason had not left me.

The whole train of symptoms strongly suggests some form of intoxication, and is not dissimilar to that produced by alcoholic excess. Here it may be noted that, as far as isolated nerves are concerned, there is pretty good evidence that alcohol and want of oxygen produce exactly the same effects, *i.e.* they cause a decrement in the conducting power of the nerve. And herein lies a part of its interest, for pharmacologists of one school at all events tell me that the corresponding effects of alcohol are really due to an inhibition of the higher centres of the mind; you can therefore conceive of the mental mechanism of self-control being knocked out either because it has not oxygen enough with which to 'carry on,' or because it is drugged by some poison as a secondary result of the anoxæmia.

And now to pass to the results of more chronic anoxæmia. If I were to try to summarise them in a sentence I should say that, just as acute anoxæmia simulates drunkenness, chronic anoxæmia simulates fatigue.

The following slide shows you a photograph taken from a page in my note-book written at the Alta Vista Hut, at an altitude of 12,000 feet. You will see that the page commences with a scrawl which is crossed out, then '6 Sept,' the word 'Sept.' is crossed out and 'March' is inserted, 'March' shares the same fate as 'Sept.,' and 'April,' the correct month, is substituted, and so on, more crossings out and corrections. All this you might say with justice is the action of a tired man. The other pages written at lower altitudes do not, however, bear out the idea that I was out of health at the time, and there was no reason for tiredness on that particular day. Another symptom frequently associated with mental fatigue is irritability. Anyone who has experience of high altitudes knows to his cost that life does not run smoothly at 10,000 feet. If the trouble is not with one's own temper, it is with those of one's colleagues: and so it was in many cases of gas-poisoning and in the case of aviators. In these subjects the apparent fatigue sometimes passed into a definitely neurasthenic condition. At this point an issue appeared to arise between the partisans of two theories. One camp said that the symptoms were definitely those of anoxæmia, the other that they were due to nerve strain. As I have indicated later on, it is not clear that these two views are mutually exclusive. It takes two substances to make an oxidation, the oxygen and the oxidised material. If the oxidation does not take place, the cause may lie in the absence of either or of both, in each case with a similar effect. The subject really is not ripe for controversy, but it is amply ripe for research, research in which both the degree of anoxæmia and the symptoms of fatigue are clearly defined.

So much, then, for the injury to the machine wrought by chronic anoxæmia.

*Types of Anoxæmia.*

And now to pass to the consideration of the various types of anoxæmia.

*Anoxæmia* is by derivation want of oxygen in the blood.

Suppose you allow your mind to pass to some much more homely substance than oxygen—such, for instance, as milk—and consider the causes which may conspire to deprive your family of milk, three obvious sources of milk deficiency will occur to you at once:

- (1) There is not enough milk at the dairy;
- (2) The milk is watered or otherwise adulterated so that the fluid on sale is not really all milk; and
- (3) The milkman from that particular dairy does not come down your road.

These three sources of milk deficiency are typical of the types of oxygen deficiency.

The first is insufficient oxygen dispensed to the blood by the lungs. An example of this type of anoxæmia is mountain-sickness. The characteristic of it is insufficient *pressure* of oxygen in the blood. In mountain-sickness the insufficiency of pressure in the blood is due to insufficient pressure in the air, for, according to our view at all events, the pressure in the blood will always be less than that in air. But this type of anoxæmia may be due to other causes. The sufferer may be in a normal atmosphere, yet for one reason or another the air may not have access to his whole lung. In such cases, either caused by obstruction, by shallow respiration, or by the presence of fluid in the alveoli, the blood leaving the affected areas will contain considerable quantities of reduced hæmoglobin. This will mix with blood from unaffected areas which is about 95 per cent. saturated. The oxygen will then be shared round equally among the corpuscles of the mixed blood, and if the resultant is only 85-90 per cent. saturated the pressure of oxygen will only be about half the normal, and, as I said, deficiency of oxygen pressure is the characteristic of this type of anoxæmia.

The second type involves no want of oxygen pressure in the arterial blood; it is comparable to the watered milk: the deficiency is really in the quality of the blood and not in the quantity of oxygen to which the blood has access. The most obvious example is anæmia, in which from one cause or other the blood contains too low a percentage of hæmoglobin, and because there is too little hæmoglobin to carry the oxygen too little oxygen is carried. Anæmia is, however, only one example which might be given of this type of anoxæmia. There may be sufficient hæmoglobin in the blood, but the hæmoglobin may be useless for the purpose of oxygen transport; it may be turned in part into methæmoglobin, as in several diseases, *e.g.* among workers in the manufacture of some chemicals, and in some forms of dysentery contracted in tropical climates, or it may be monopolised by carbon monoxide, as in mine-air.

Thirdly, the blood may have access to sufficient oxygen and may contain sufficient functional hæmoglobin, but owing to transport trouble it may not be circulated in sufficient quantities to the tissues. The quantity of oxygen which reaches the tissue in unit time is too small.

Literally, according to the strict derivation of the word 'anoxæmia,' the third type should perhaps be excluded from the category of conditions covered by that word, but, as the result is oxygen starvation in the tissues, it will be convenient to include it. Indeed, it would be an act of pedantry not to do so, for no form of anoxæmia has any significance apart from the fact that it prevents the tissues from obtaining the supply of oxygen requisite for their metabolic processes.

The obvious types of anoxæmia may therefore be classified in some such scheme as the following, and as it is difficult to continue the discussion of them without some sort of nomenclature, I am giving a name to each type:

#### ANOXÆMIA.

1. Anoxic Type	2. Anæmic Type.	3. Stagnant Type.
The pressure of oxygen in the blood is too low.	The quantity of functional hæmoglobin is too small.	The blood is normal, but is supplied to the tissues in insufficient quantities.
The hæmoglobin is not saturated to the normal extent.	The oxygen pressure is normal.	Examples:
The blood is dark.	The blood is normal in colour.	1. Secondary result of histamine shock.
Examples:	Examples:	2. Hæmorrhage.
1. Rare atmospheres.	1. Too little hæmoglobin.	3. Back pressure.
2. Areas of lung partially unventilated.	2. CO hæmoglobin.	
3. Fluid or fibrin on surface of cells.	3. Methæmoglobin.	

Anoxic anoxæmia is essentially a general as opposed to a local condition. Not only is the pressure of oxygen in the blood too low, but the lowness of the pressure and not the deficiency in the quantity is the cause of the symptoms observed.

Proof of the above statement is to be found in the researches of most workers who have carried out investigations at low oxygen pressures, and it can now be brought forward in a much more convincing way than formerly that oxygen secretion is, for the time at all events, not a factor to be counted with.

The workers on Pike's Peak, for instance, emphasised the fact that the increase of red blood corpuscles during their residence at 14,000 feet was due to deficient oxygen pressure. No doubt they were right, but the point was rather taken from their argument by their assertion in another part of the paper that the oxygen pressure in their arterial blood was anything up to about 100 mm. of mercury. Let me therefore take my own case, in which the alveolar pressures are known to be an index of the oxygen pressures in the arterial blood. I will compare my condition on two occasions, the point being that on these two occasions the quantities of oxygen united with the hæmoglobin were as nearly as may be the same, whilst the pressures were widely different.

As I sit here the hæmoglobin value of my blood is 96.97, which corresponds to an oxygen capacity of .178 c.c. of O<sub>2</sub> per c.c. of blood. In the oxygen chamber on the last day of my experiment, to which I refer later,<sup>5</sup> the oxygen capacity of my blood was .201 c.c. Assuming the blood to be 95 per cent. saturated now and 84 per cent. saturated

then, the actual quantity of oxygen in the blood on the two occasions would be :

Oxygen Capacity.	Percentage Saturation.	Oxygen Content.
·178	95	·169
·201	84	·169

Here, I am in my usual health. In the chamber, I vomited, my pulse was 86—it is now 56; my head ached in a most distressing fashion, it was with the utmost difficulty that I could carry out routine gas analyses, and when doing so the only objects which I saw distinctly were those on which my attention was concentrated.

In the anoxic type of anoxæmia there may then be quite a sufficient quantity of oxygen in the blood, but a sufficient quantity does not avail in the face of an insufficient pressure. Indeed, as I shall show presently, the anoxic type of anoxæmia is the most serious. We are therefore confronted with something of a paradox in that the most severe type of anoxæmia is one in which there is not necessarily an insufficient quantity of oxygen in the blood at all.

And here let me justify the statement that the anoxic type is the most to be feared. I can justify it on either or both of two grounds. Firstly, of the three types it places the tissues at the greatest disadvantage as regards oxygen supply, and secondly, it is of the three the least easy type for the organism to circumvent.

Let me dwell for a moment upon the efficiency of the blood as a medium for the supply of oxygen to the tissue in the three types. The goal of respiration is to produce and maintain as high an oxygen pressure in the tissue fluids as possible. For the velocity of any particular oxidation in the tissues must depend upon the products of the concentrations (active masses) of the material to be oxidised and of the oxygen. Now the concentration of oxygen in the tissue is proportional to its partial pressure, and the highest partial pressure in the tissue must, other things being equal, be the result of that type of anoxæmia in which there is the highest partial pressure in the blood plasma.

It is interesting and not uninteresting to try to calculate the degree to which the tissues are prejudiced by being subjected to various types of anoxæmia. Let us suppose that we have a piece of tissue, muscle for instance, which normally is under the following conditions :

- (a) One cubic centimetre of blood per minute runs through it.
- (b) The total oxygen capacity of this blood is ·188 c.c. of oxygen per c.c. of blood.
- (c) The percentage saturation is 97.
- (d) The oxygen pressure is 100 mm.
- (e) The oxygen used is ·059 c.c.
- (f) The oxygen pressure in the tissues is half of that in the veins, in this case 19 mm.

Compare with this a severe case of anoxic anoxæmia, one in which the blood-flow is the same as above, and also the oxygen capacity value of the blood, but in which the oxygen pressure is only 31 mm. and the percentage saturation of the arterial blood 66 per cent. Let us further retain the assumption that the oxygen pressure in the tissues is half

that in the veins. It is possible to calculate, as indeed has been done by my colleague, Mr. Roughton, what the amount of oxygen leaving the capillaries is. The answer is not '059, as in the case of the normal, but '026—less than half the normal. So, other things being equal, cutting down the oxygen pressure in the arterial blood to 31 and the percentage saturation to 66 would deprive the tissues of half their oxygen. With this compare an example of the anæmic type. The arterial blood shall have the same total quantity of oxygen as in the anoxic case, but instead of being 66 per cent. saturated it shall contain 66 per cent. of the total hæmoglobin, which shall be normally saturated. The amount of oxygen which would pass to the tissues under these conditions is '041 c.c.—more than half as much again as from the anoxic blood laden with the same quantity of oxygen.

And thirdly, let us take for comparison a case of stagnant anoxæmia in which the same quantity of oxygen goes to the tissue in the cubic centimetre of blood as in the anoxic and anæmic types. On the assumption which we have made the quantity of oxygen which would leave the blood would be '045 c.c.

In round numbers therefore the prejudice to the tissues may be expressed by the following comparison. In this case both of the oxygen going to the blood and going into the tissues I have called the normal 100. This does not, of course, mean that the two amounts are the same. The former in absolute units is about three times the latter. The figure 100 at the top of each column is merely a standard with which to compare the figures beneath it.

	Oxygen in blood going to vessels of tissue. Per cent.	Oxygen leaving the blood to supply the tissues. Per cent.
Normal	100	100
Anoxæmia	Anoxic . . . 66	42
	Anæmic . . . 66	66
	Stagnant . . . 66	75

#### *Measurement of Anoxæmia.*

In the study of all physical processes there comes a point, and that very early, when it becomes necessary to compare them one with another, to establish some sort of numerical standard and have some sort of quantitative measurements. The study of anoxæmia has reached that point. By what scale are we to measure oxygen want?

Let us take the anoxic type first. There are two scales which might be applied to it, both concern the arterial blood; the one is the oxygen pressure in it, the other is the actual percentage of the hæmoglobin which is oxyhæmoglobin. A third possibility suggests itself, namely, the actual amount of oxygen present, but this would be influenced as much by the anæmic as by the anoxic conditions. Of the two possibilities—that of measuring the pressure, and that of measuring the saturation of the blood with oxygen—the latter is the one which is likely to come into vogue, because it is susceptible of direct measurement.

Two converging lines of work have within the last few years brought us nearer to being able to state the degree of anoxæmia in man in terms of the percentage saturation of oxygen in his blood. The first, intro-

duced by the American researcher Stadie,<sup>6</sup> is the method of arterial puncture. It had long been the wish of physiologists to make direct examinations of the gases in human arterial blood, yet, as far as I know, this had only once been accomplished, namely, by Dr. Arthur Cooke and myself in a case in which the radial artery was opened for the purpose of transfusion. But the matter now seems to be relatively simple. The needle of a hypodermic syringe can be put right into the radial artery and arterial blood withdrawn. I am not sure that the operation is less painful than that of dissecting out the radial artery and opening it—and in this matter I speak with experience—but it is less alarming, and it has the great merit that it does not injure the artery.

Another method of determining the percentage saturation of arterial blood has invited the attention of researchers, appearing like a will-o'-the-wisp, at one time within grasp, at another far off. That method is to deduce the percentage saturation from the composition of the alveolar air. Into the merits of the rival methods for the determination of the oxygen in alveolar air I will not go: the method of Haldane and Priestley will suffice for persons at rest. Granted, then, that a subject has a partial pressure of 50 mm. of oxygen in his alveolar air, what can we infer as regards his arterial blood? A long controversy has raged about whether or no any assumption could be made about the condition of the arterial blood from that of the alveolar air, for it was an article of faith with the school of physiologists which was led by Haldane that when the oxygen pressure in the alveolar air sank, the oxygen in the arterial blood did not suffer a corresponding reduction. The experimental evidence at present points in the opposite direction, and unless some further facts are brought to light it may be assumed that the oxygen pressure in the arterial blood of a normal person at rest is some five millimetres below that in his alveolar air. And having obtained a figure for the pressure of oxygen in the arterial blood, where do we stand as regards the percentage saturation? The relation between the one and the other is known as the oxygen dissociation curve. It differs but slightly in normal individuals, and at different times in the same individual. To infer the percentage saturation from the oxygen pressure, no doubt the actual dissociation curve should be determined, but in practice it is doubtful whether as a first approximation this is necessary, for a curve determined as the result of a few observations is unlikely to be much nearer the mark than a standard curve on which twenty or thirty points have been determined. Therefore an approximation can be made for the percentage saturation as follows: In a normal individual take the oxygen in the alveolar air, subtract five millimetres, and lay the result off on the mean dissociation curve for man.

Whether measured directly or indirectly, the answer is a statement of the relative quantities of oxyhæmoglobin and of reduced hæmoglobin in the arterial blood. The important thing is that there should be as little reduced hæmoglobin as possible. The more reduced hæmoglobin there is present the less saturated is the blood, or, as the American authors say, the more *unsaturated* is the blood. They emphasise

the fact that it is the quantity of reduced hæmoglobin that is the index of the anoxic condition. They speak not of the percentage saturation, but of the percentage of unsaturation. A blood which would ordinarily be called 85 per cent. saturated they speak of as 15 per cent. unsaturated.

Anoxic anoxæmia, in many cases of lung affection, should be measured by the direct method of arterial puncture, for the simple reason that the relation between the alveolar air and the arterial blood is quite unknown. Such, for instance, are cases of many lung lesions of pneumonia in which the lung may be functioning only in parts, of pneumothorax, of pleural effusions, of emphysema, of multiple pulmonary embolism, in phases of which the arterial blood has been found experimentally to be unsaturated. In addition to these definite lung lesions, there is another type of case on which great stress has been laid by Haldane, Meakins, and Priestley, namely, cases of shallow respiration.<sup>7</sup> A thorough investigation of the arterial blood in such cases is urgently necessary. Indeed, in all cases in which it is practicable, the method of arterial puncture is desirable. But in the cases of many normal persons—such, for instance, those airmen at different altitudes—alveolar-air determinations would give a useful index.

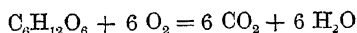
The anæmic type of anoxæmia is gauged by the quantity of oxy-hæmoglobin in the blood. In the case of simple anæmias this is measured by the scale in which the normal man counts as 100 and the hæmoglobin in the anæmic individual is expressed as a percentage of this. This method has been standardised carefully by Haldane, and we now know that the man who shows 100 on the scale has an oxygen capacity of 185 c.c. of oxygen for every c.c. of blood. We can therefore, in cases of carboxy-hæmoglobin, or methæmoglobin poisoning, express the absolute amount of oxyhæmoglobin pressure either by stating the oxygen capacity and so getting an absolute measurement, or in relative units by dividing one hundred times the oxygen capacity by 185, and thus getting a figure on the ordinary hæmoglobin metre scale.

### *The Mechanism of Anoxæmia.*

Perhaps the most difficult phase of the discussion is that of how anoxæmia produces its baneful results. In approaching this part of the subject I should like to warn my readers of one general principle the neglect of which seems to be responsible for a vast dissipation of energy. Before you discuss whether a certain effect is due to cause A or cause B, be clear in your own mind that A and B are mutually exclusive.

Let me take an example and suppose

- (1) That the energy of muscular contraction in the long run depends in some way on the oxidation of sugar;
- (2) That in the absence of an adequate supply of oxygen the reaction



cannot take place in its entirety;

- (3) That under such circumstances some lactic acid is formed as well as carbonic acid;



- (4) That the hydrogen ion concentration of the blood rises and the total ventilation increases. On what lines are you to discuss whether the increased ventilation is due to 'acidosis,' by which is meant in this connection the increased hydrogen ion concentration of the blood, or to 'anoxæmia'? Clearly not on the lines that it must be due to one or other.

In the above instance anoxæmia and acidosis are to some extent dependant variables. I have chosen the above case because measurements have been made throughout which make the various assumptions fairly certain, and tell us pretty clearly in what sort of chain to string up the events, what is cause and what is effect. Clearly it would be ridiculous to start a discussion as to whether the breathlessness was due to 'acidosis' or 'anoxæmia'. Each has its place in the chain of events. But I have heard discussions of whether other phenomena of a more obscure nature were due to oxygen want or to acidosis. Such discussions tend to no useful end.

Nor is this the only problem with regard to oxygen want concerning which my warning is needed. Oxygen want may act immediately in at least two ways:

- (1) In virtue of absence of oxygen some oxidation which otherwise might take place does not do so, and therefore something which might otherwise happen may not happen. For instance, it may be conceived that the respiratory centre can only go through the rhythmic changes of its activity as the result of the oxidation of its own substance.
- (2) A deficient supply of oxygen may produce, not the negation of a chemical action, but an altered chemical action which in its turn produces toxic products that have a secondary effect on such an organism as the respiratory centre.

Now these effects are not mutually exclusive. In the same category are many arguments about whether accumulations of carbonic acid act specifically as such or merely produce an effect in virtue of their effect on the hydrogen ion concentration.. Here again the two points of view are not, strictly speaking, alternatives, and, in some cases at all events, both actions seem to go on at the same time.

It will be evident that in any balanced action in which  $\text{CO}_2$  is produced its accumulation will tend to slow the reaction; but, on the other hand, the same accumulation may very likely raise the hydrogen ion concentration, and in that way produce an effect.

The relation of oxygen to hæmoglobin seems to furnish a case in point. Carbonic acid is known to reduce the affinity of hæmoglobin for oxygen, and other acids do the same. On analogy, therefore, it might have, and has, been plausibly argued that  $\text{CO}_2$  acts in virtue of the change in reaction which it produces. Put into mathematical language, the relation of the percentage saturation of oxygen to the oxygen pressure of the gas dissolved in the hæmoglobin solution is expressed by the equation

$$\frac{y}{100} = \frac{Kx^n}{1 + Kx^n} = \frac{x^n}{L + x^n}, \text{ where } L = \frac{1}{K}$$

where  $y$  is the percentage saturation and  $x$  the oxygen pressure. The value of  $K$  is the measure of the affinity of oxygen for hæmoglobin: the less the value of  $K$  the less readily do the two substances unite.

Now  $\frac{1}{K}$  has been shown by Laurence J. Henderson,\* and independently by Adair, to vary directly with the concentration of  $\text{CO}_2$ . The value of this constant is, according to Henderson, too great to be a direct effect of the  $\text{CO}_2$  on the hæmoglobin, and involves as well the assumption that the hæmoglobin in blood is in four forms—an acid and a salt of reduced hæmoglobin and an acid and a salt of oxyhæmoglobin. The presence of  $\text{CO}_2$  alters the balance of these four substances.

It is rather fashionable at present to say that 'the whole question of acidosis and anoxæmia is in a hopeless muddle.' To this I answer that, if it is in a muddle, I believe the reason to be largely because schools of thought have rallied round words and have taken sides under the impression that they have no common ground. The 'muddle,' in so far as it exists, is not, I think, by any means hopeless; but I grant freely enough that we are rather at the commencement than at the end of the subject, that much thought and much research must be given, firstly, in getting accurate data, and, secondly, on relating cause and effect, before the whole subject will seem simple. No effort should be spared to replace indirect by direct measurements. My own inference with regard to changes of the reaction of the blood, based on interpretations of the dissociation curve, should be checked by actual hydrogen ion measurements, as has been done by Hasselbach and is being done by Donegan and Parsons.<sup>9</sup> Meakins also is, I think, doing great work by actually testing the assumptions made by Haldane and himself as regards the oxygen in arterial blood.

### *The Compensations for Anoxæmia.*

For the anoxic type of anoxæmia two forms of compensation at once suggest themselves. The one is increased hæmoglobin in the blood; the other is increased blood-flow through the tissues. Let us, along the lines of the calculations already made, endeavour to ascertain how far these two types of compensation will really help. To go back to the extreme anoxic case already cited, in which the hæmoglobin was 66 per cent. saturated, let us, firstly, see what can be accomplished by an increase of the hæmoglobin value of the blood. Such an increase takes place, of course, at high altitudes. Let us suppose that the increase is on the same grand scale as the anoxæmia, and that it is sufficient to restore the actual quantity of oxygen in one c.c. of blood to the normal. This, of course, means a rise in the hæmoglobin value of the blood from 100 to 150 on the Gowers' scale. Yet even so great an increase in the hæmoglobin will only increase the oxygen taken up in the capillary from each c.c. of blood from '031 to '036 c.c., and will therefore leave it far short of the '06 c.c. which every cubic centimetre of normal blood was giving to the tissue. So much, then, for increased hæmoglobin. It gives a little, but only a little, respite. Let us turn, therefore, to increased blood-flow.

In the stagnant type of anoxæmia the principal change which is seen to take place is an increase in the quantity of hæmoglobin per cubic millimetre of blood.

This increase is secondary to a loss of water in the tissues, the result in some cases, as appears from the work of Dale, Richards, and Laidlaw,<sup>10</sup> of a formation of histamine in the tissues. Whether this increase of hæmoglobin is to be regarded as merely an accidental occurrence or as a compensation is difficult to decide at present. Roughton's calculations rather surprised us by indicating that increased hæmoglobin acted less efficiently as a compensatory mechanism than we had expected. This conclusion may have been due to the inaccuracy of our assumptions. I must therefore remind you that much experimental evidence is required before the assumptions which are made above are anything but assumptions. But, so far as the evidence available at the present time can teach any lesson, that lesson is this: The only way of dealing satisfactorily with the anoxic type of anoxæmia is to abolish it by in some way supplying the blood with oxygen at a pressure sufficient to saturate it to the normal level.

It has been maintained strenuously by the Oxford school of physiologists that Nature actually did this; that when the partial pressure in the air-cells of the lung was low the cellular covering of that organ could clutch at the oxygen and force it into the blood at an unnatural pressure, creating a sort of forced draught. This theory, as a theory, has much to recommend it. I am sorry to say, however, that I cannot agree with it on the present evidence. I will only make a passing allusion to the experiment which I performed in order to test the theory, living for six days in a glass respiration-chamber in which the partial pressure of oxygen was gradually reduced until it was at its lowest—about 45 mm. Such a pressure, if the lung was incapable of creating what I have termed a forced draught, would mean an oxygen pressure of 38-40 mm. of mercury in the blood, a change sufficient to make the arterial blood quite dark in colour, whereas, did any considerable forced draught exist, the blood in the arteries would be quite bright in colour. Could we but see the blood in the arteries, its appearance alone would almost give the answer as to whether or no oxygen was forced, or, in technical language, secreted, through the lung wall. And, of course, we could see the blood in the arteries by the simple process of cutting one of them open and shedding a little into a closed glass tube. To the surgeon this is not a difficult matter, and it was, of course, done. The event showed that the blood was dark, and the most careful analyses failed to discover any evidence that the body can force oxygen into the blood in order to compensate for a deficiency of that gas in the air.

Yet the body is not quite powerless. It can, by breathing more deeply, by increasing the ventilation of the lungs, bring the pressure of oxygen in the air-cells closer to that in the atmosphere breathed than would otherwise be the case. I said just now that the oxygen in my lungs dropped to a minimal pressure of 45 mm.; but it did not remain at that level. When I bestirred myself a little it rose, as the result of increased ventilation of the lung, to 56 mm., and at one time, when I was breathing through valves, it reached 68 mm. Nature will

do something, but what Nature does not do should be done by artifice. Exploration of the condition of the arterial blood is only in its infancy, yet many cases have been recorded in which in illness the arterial blood has lacked oxygen as much as or more than my own did in the respiration chamber when I was lying on the last day, with occasional vomiting, racked with headache, and at times able to see clearly only as an effort of concentration. A sick man, if his blood is as anoxic as mine was, cannot be expected to fare better as the result, and so he may be expected to have all my troubles in addition to the graver ones which are, perhaps, attributable to some toxic cause. Can he be spared the anoxæmia? The result of our calculations, so far, points to the fact that the efficient way of combating the anoxic condition is to give oxygen. During the war it was given with success in the field in cases of gas-poisoning, and also special wards were formed on a small scale in this country in which the level of oxygen in the atmosphere was kept up to about 40 per cent., with great benefit to a large percentage of the cases. The practice then inaugurated is being tested at Guy's Hospital by Dr. Hunt, who administered the treatment during the war.

Nor are the advantages of oxygen respiration confined to pathological cases. One of the most direct victims of anoxic anoxæmia is the airman who flies at great heights. Everything in this paper tends to show that to counteract the loss of oxygen which he sustains at high altitudes there is but one policy, namely, to provide him with an oxygen equipment which is at once as light and as efficient as possible—a consummation for which Haldane has striven unremittingly. And here I come to the personal note on which I should like to conclude. In the pages which I have read views have been expressed which differ from those which he holds in matters of detail—perhaps in matters of important detail. But Haldane's teaching transcends mere detail. He has always taught that the physiology of to-day is the medicine of to-morrow. The more gladly, therefore, do I take this opportunity of saying how much I owe, and how much I think medicine owes and will owe, to the inspiration of Haldane's teaching.

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# British Association for the Advancement of Science.

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SECTION K: CARDIFF, 1920.

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## ADDRESS TO THE BOTANICAL SECTION

BY  
Miss E. R. SAUNDERS, F.L.S.,  
PRESIDENT OF THE SECTION.

YEAR by year we see the meetings of the Association recur, pursuing a course which neither geographer nor astronomer would venture to predict and leaving traced out behind them a figure unknown to the mathematician. Nevertheless the path of its journeyings is ever returning upon itself. As this recurrence is brought afresh to one's mind, there is a natural impulse to reflect upon the progress which has been made in the intervening period in the science which one here finds oneself called upon to represent. Not quite thirty years have elapsed since the last occasion on which the Association was welcomed to Cardiff. Curiosity to learn whether the matter of the discourse delivered by my predecessor on that occasion had a connection, close or remote, with the particular subject with which I proposed to deal in this Address led me to refer to the Annual Report of the Association for 1891. I thus became aware how recent was the occurrence of the mutation—or should I rather say of the dichotomy?—which led to the appearance of a Botanical Section, for twenty-nine years ago Section K had not yet come into existence. At that period the problems relating to living organisms, whether concerned with plant or animal, whether of a morphological or physiological nature, were all embraced within the wide field of Section D, the Section of Biology. Though in succeeding years discovery at an ever-increasing rate and in many new fields of investigation has made inevitable the separation first of Physiology, and then of Botany from their common parent, we may with advantage follow the precedent set by the Association as a whole, and, as a Section, return from time to time upon our course of evolution. I shall therefore invite your attention to a subject which lies within the wide province of Biology and makes its appeal alike to the botanist, zoologist and physiologist—the subject of Heredity.

By the term Inheritance we are accustomed to signify the obvious fact of the resemblance displayed by all living organisms between offspring and parents, as the direct outcome of the contributions received from the two sides of the pedigree at fertilisation: to indicate, in fact,

owing to lack of knowledge of the workings of the hereditary process, merely the *visible* consequence—the final result of a chain of events. Now, however, that we have made a beginning in our analysis of the stages which culminate in the appearance of any character, a certain looseness becomes apparent in our ordinary use of the word Heredity, covering as it does the two concomitant essentials, genetic potentiality and somatic expression—a looseness which may lead us into the paradoxical statement that inheritance is wanting in a case in which nevertheless the evidence shows that the genetic constitution of the children is precisely like that of the parents. When we say that a character is inherited no ambiguity is involved, because the appearance of the character entails the inheritance of the genetic potentiality. But when a character is stated not to be inherited it is not thereby indicated whether this result is due to environmental conditions, to genetic constitution, or to both causes combined. That we are now able in some measure to analyse the genetic potentialities of the individual is due to one of those far-reaching discoveries which change our whole outlook, and bring immediately in their train a rapidly increasing array of new facts, falling at once into line with our new conceptions, or by some orderly and constant discrepancy pointing a fresh direction for attack. An historical survey of the steps by which we have advanced to the present state of our knowledge of Heredity has so frequently been given during the last twenty years that the briefest reference to this part of my subject will suffice.

The earliest attempts to frame some general law which would co-ordinate and explain the observed facts of inheritance were those of Galton and Pearson. Galton's observations led him to formulate two principles which he believed to be capable of general application—the Law of Ancestral Heredity and the Law of Regression. The Law of Ancestral Heredity was intended to furnish a general expression for the sum of the heritage handed on in any generation to the succeeding offspring. Superposed upon the working of this law were the effects of the Law of Regression, in which the average deviation from the mean of a whole population of any fraternal group within that population was expressed in terms of the average deviation of the parents. These expressions represent statements of averages which, in so far as they apply, hold only when large numbers are totalled together. They afford no means of certain prediction in the individual case. These and all similar statistical statements of the effects of inheritance take no account of the essentially physiological nature of this as of all other processes in the living organism. They leave us unenlightened on the fundamental question of the nature of the means by which the results we witness came to pass. We obtain from them, as from the melting-pot, various new products whose properties are of interest from other viewpoints, but, corresponding to no biological reality, they have failed to bring us nearer to our goal—a fuller comprehension of the workings of the hereditary mechanism. Progress in this direction has resulted from the opposite method of inquiry—the study of a single character in a single line of descent, the method which deals with the unit in place of the mass. The revelation came with the opening of the present

century, for in 1900 was announced the rediscovery of Mendel's work, actually given to the world thirty-five years earlier, but at the time leaving no impress upon scientific thought. The story of the Austrian monk and the details of his experiments carried out in the monastery garden upon races of the edible pea are now familiar history, and I need not recount them here. Having formed the idea that in order to arrive at a clearer understanding of the relation of organisms to their progeny the problem must be studied in its simplest form, Mendel came to see that a scheme of analysis must deal not with mass populations but with a smaller unit—the family, and that each character of the individual must be separately investigated.

Selecting for his experiments races which showed themselves to be pure-breeding and mating together those exhibiting characters of such opposite nature as to constitute a pair—*e.g.*, tall with short, yellow-seeded with green-seeded—he obtained results which could be accounted for if it were supposed that these opposite, or as we should now term them allelomorphic, characters were distributed *unaltered and in equal proportion* to the reproductive cells of the cross-bred organism. It is this conception of the pure nature of the germ-cells, irrespective of whether the organism forming them be of pure-bred or cross-bred descent, which revolutionised our conceptions of Heredity and laid the foundations upon which we build to-day. For the intervening years have seen the instances in which the Mendelian theory is found to hold mount steadily from day to day, furnishing a weight of evidence in its support which is incontrovertible.

It chanced that in each pair of characters selected by Mendel for experiment the opposites are related to each other in the following simple manner: An individual which had received both allelomorphs, one from either parent, exhibited one of the two characteristics, hence called the dominant, to the exclusion of the other. Among the offspring of such an individual both characteristics appeared, the dominant in some, its opposite, the recessive, in others, in the proportion approximately of three to one. This is the result which might be expected from random pairing in fertilisation of two opposites, where the manifestation in the zygote of the one completely masks the presence of the other. As workers along Mendelian lines increased and the field of inquiry widened, it soon, however, became apparent that the dominant-recessive relationship is not of universal occurrence. It likewise became clear that the simple ratios which obtained in Mendel's experiments are not characteristic of every case. Mendel's own results were all, as it happened, explicable on the supposition that the two alternative forms of each character were dependent on a *single* element or factor. By a fortunate accident none of the complex factorial interrelations which have since been brought to light in other cases obscured the expression in its simplest form of the results of germ purity. It is our task, in the light of this guiding principle, to attempt to elucidate these more complicated types of inheritance.

We now know, for example, that many characters are not controlled by one single factor, but by two or more. One of the most familiar instances of the two-factor character is the appearance of the



colouring matter anthocyanin in the petals of plants such as the Stock and Sweet Pea. Our proof that two factors (at least) are here involved is obtained when we find that two true breeding forms devoid of colour yield coloured offspring when mated together. In this case the two complementary factors are carried, one by each of the two crossed forms. When both factors meet in the one individual, colour is developed. We have in such cases the solution of the familiar, but previously unexplained, phenomenon of Reversion. Confirmatory evidence is afforded when among the offspring of such cross-bred individuals we find the simple 3 to 1 ratio of the one-factor difference replaced by a ratio of 9 to 7. Similarly we deduce from a ratio of 27 to 37 that three factors are concerned, from a ratio of 81 to 175 four factors, and so on. The occurrence of these higher ratios proves that the hereditary process follows the same course whatever the number of factors controlling the character in question.

And here I may pause to dwell for a moment upon a point of which it is well that we should remind ourselves from time to time, since, though tacitly recognised, it finds no explicit expression in our ordinary representation of genetic relations. The method of factorial analysis based on the results of inter-breeding enables us to ascertain the least possible number of genetic factors concerned in controlling a particular somatic character, but what the total of such factors actually is we cannot tell, since our only criterion is the number by which the forms we employ are found to differ. How many may be common to these forms remains unknown. In illustration I may take the case of surface character in the genera *Lychnis* and *Matthiola*. In *L. vespertina* the type form is hairy; in the variety *glabra*, recessive to the type, hairs are entirely lacking. Here all glabrous individuals have so far proved to be similar in constitution, and when bred with the type give a 3 to 1 ratio in  $F_2$ .<sup>1</sup> We speak of hairiness in this case, therefore, as being a one-factor character. In the case of *Matthiola incana* v. *glabra*, of which many strains are in cultivation, it so happened that the commercial material originally employed in these investigations contained all the factors since identified as present in the type and essential to the manifestation of hairiness except one. Hence it appeared at first that here also hairiness must be controlled, as in *Lychnis*, by a single factor. But further experiment revealed the fact that though the total number of factors contained in these glabrous forms was the same, the respective factorial combinations were not identical. By inter-breeding these and other strains obtained later, hairy  $F_1$  cross-breds were produced giving ratios in  $F_2$  which proved that at least four distinct factors are concerned.<sup>2</sup> Whereas, then, the glabrous appearance in *Lychnis* always indicates the loss (if for convenience we may so represent the nature of the recessive condition) of one and the same factor, analysis in the Stock shows that the glabrous condition results if any factor out of a group of four is represented by its recessive allelomorph. Hence we describe hairiness in the latter case as a four-factor character.

<sup>1</sup> Report to the Evolution Committee, Royal Society, i., 1902.

<sup>2</sup> Proc. Roy. Soc. B, vol. 85, 1912.

It will be apparent from the cases cited that we cannot infer from the genetic analysis of one type that the factorial relations involved are the same for the corresponding character in another. That this should be so in wholly unrelated plants is not perhaps surprising, but we find it to be true also where the nature of the characteristic and the relationship of the types might have led us to expect uniformity. This is well seen in the case of a morphological feature distinctive of the N.O. Gramineæ. The leaf is normally ligulate, but individuals are occasionally met with in which the ligule is wanting. In these plants, as a consequence, the leaf blade stands nearly erect instead of spreading out horizontally. Nilsson-Ehle<sup>3</sup> discovered that in Oats there are at least four and possibly five distinct factors determining ligule formation, all with equal potentialities in this direction. Hence, only when the complete series is lacking is the ligule wanting. In mixed families the proportion of ligulate to non-ligulate individuals depends upon the number of these ligule-producing factors contained in the dominant parent. Emerson<sup>4</sup> found, on the other hand, that in Maize mixed families showed constantly a 3 to 1 ratio, indicating the existence of only one controlling factor.

From time to time the objection has been raised that the Mendelian type of inheritance is not exhibited in the case of specific characters. That no such sharp line of distinction can be drawn between the behaviour of varietal and specific features has been repeatedly demonstrated. As a case in point and one of the earliest in which clear proof of Mendelian segregation was obtained, we may instance *Datura*. The two forms, *D. Stramonium* and *D. Tatula*, are ranked by all systematists as distinct species. Among other specific differences is the flower colour. The one form has purple flowers, the other pure white. In the case of both species a variety *inermis* is known in which the prickles characteristic of the fruit in the type are wanting. It has been found that in whatever way the two pairs of opposite characters are combined in a cross between the species, the F<sub>2</sub> generation is mixed, comprising the four possible combinations in the proportions which we should expect in the case of two independently inherited pairs of characters, when each pair of opposites shows the dominant-recessive relation. Segregation is as sharp and clean in the specific character flower colour as in the varietal character of the fruit. Among the latest additions to the list of specific hybrids showing Mendelian inheritance, those occurring in the genus *Salix* are of special interest; since heretofore the data available had been interpreted as conflicting with the Mendelian conception. The recent observations of Heribert-Nilsson<sup>5</sup> show that those characters which are regarded by systematists as constituting the most distinctive marks of the species are referable to an extremely simple factorial system, and that the factors mendelise in the ordinary way. Furthermore, these specific-character factors

<sup>3</sup> *Kreuzungsuntersuchungen an Hafer und Weizen*, Lund, 1909.

<sup>4</sup> Annual Report of the Agricultural Experiment Station of the University of Nebraska, 1912.

<sup>5</sup> *Experimentelle Studien über Variabilität, Spaltung, Arthildung und Evolution in der Gattung Salix*, 1918.

control not only the large constant *morphological features*, but *fundamental reactions* such as those determining the condition of physiological equilibrium and vitality in general. In so far as any distinction can be drawn between the behaviour of factors determining the varietal as opposed to the specific characters of the systematist, Heribert-Nilsson concludes that the former are more localised in their action, while the latter produce more diffuse results, which may affect almost all the organs and functions of the individual, and thus bring about striking alterations in the general appearance. *S. caprea*, for example, is regarded as the reaction product of two distinct factors which together control the leaf-breadth character, but which also affect, each separately and in a different way, leaf form, leaf colour, height, and the periodicity of certain phases. We cannot, however, draw a hard-and-fast line between the two categories. The factor controlling a varietal characteristic often produces effects in different parts of the plant. For example, the factors which lead to the production of a coloured flower no doubt also in certain cases cause the tinging seen in the vegetative organs, and affect the colour of the seed. Heribert-Nilsson suggests that fertility between species is a matter of close similarity in genotypic (factorial) constitution rather than of outward morphological resemblance. Forms sundered by the systematist on the ground of gross differences in certain anatomical features may prove to be more akin, more compatible in constitution, than others held to be more nearly related because the differentiating factors happen to control less conspicuous features.

Turning to the consideration of the more complex types of inheritance already referred to, we find numerous instances where a somatic character shows a certain degree of coupling or linkage with another perhaps wholly unrelated character. This phenomenon becomes still further complicated when, as is now known to occur fairly frequently, somatic characters are linked also with the sex character. The results of such linkages appear in the altered proportions in which the various combinations of the several characters appear on cross-breeding. Linkage of somatic characters can be readily demonstrated in the garden Stock. Some strains have flowers with deeply coloured sap, *e.g.*, full red or purple; others are of a pale shade such as a light purple or flesh colour. In most commercial strains the 'eye' of the flower is white owing to absence of colour in the plastids, but in some the plastids are cream-coloured, causing the sap colour to appear of a much richer hue and giving a cream 'eye.' Cream plastid colour is recessive to white and the deep sap colours are recessive to the pale. When a cream-eyed strain lacking the pale factor is bred with a white-eyed plant of some pale shade, the four possible combinations appear in  $F_2$  but not, as we should expect in the case of two independently inherited one-factor characters, in the proportions 9 : 3 : 3 : 1, with the double recessive as the least abundant of the four forms. We find instead that the double dominant and the double recessive are both in excess of expectation, the latter being more abundant than either of the combinations of one dominant character with one recessive. The two forms which preponderate are those which exhibit the

combinations seen in the parents, the two smaller categories are those representing the new combinations of one paternal with one maternal characteristic. In the Sweet Pea several characters are linked in this manner, viz.: flower colour with pollen shape, flower colour with form of standard, pollen shape with form of standard, colour of leaf axil with functioning capacity of the anthers. If in these cases the cross happens to be made in such a way that the two dominant characters are brought in one from each side of the pedigree instead of both being contributed by one parent, we get again a result in which the two parental combinations occur more frequently, the two recombinations or 'crossovers' less often than we should expect. In the first case the two characters appear to hang together in descent to a certain extent but not completely, in the latter similarly to repel each other. This type of relationship has been found to be of very general occurrence. The linked characters do not otherwise appear to be connected in any way that we can trace, and we therefore conclude that the explanation must be sought in the mechanism of distribution. Two main theories having this fundamental principle as their basis but otherwise distinct have been put forward, and are usually referred to as the *reduplication* and the *chromosome* view respectively. The reduplication view, proposed by Bateson and Punnett,<sup>6</sup> rests on the idea that segregation of factors need not necessarily occur simultaneously at a particular cell division. The number of divisions following the segregation of some factors being assumed to be greater than those occurring in the case of others, there would naturally result a larger number of gametes carrying some factorial combinations and fewer carrying others. If this differential process is conceived as occurring in an orderly manner it would enable us to account for the facts observed. We could imagine how it came about that gametic ratios such as 3 : 1 : 1 : 3. 7 : 1 : 1 : 7. 15 : 1 : 1 : 15, and so on arose giving the series of linkages observed. It has, however, to be said that we cannot say *why* segregation should be successive nor at what moments, on this view, it must be presumed to occur. On the other hand, the conceptions embodied in the chromosome hypothesis as formulated by Morgan and his fellow-workers<sup>7</sup> is, in these respects, quite precise. They are built around one cardinal event in the life cycle of animals and plants (some of the lowest forms excepted), viz.: the peculiar type of cell division at which the number of chromosomes is reduced to half that to be found during the period of the life cycle extending backwards from this moment to the previous act of fertilisation. In the large number of cases already investigated the chromosome number has been found as a rule to be the same at each division of the somatic cells. We can, in fact, take it as established that it is ordinarily constant for the species. These observations lend strong support to the view that the chromosomes are persistent structures, that is to say, that the chromatin tangle of the resting nucleus, whether actually composed of one continuous thread or not, becomes resolved into

<sup>6</sup> *Proc. Roy. Soc.*, 1911.

<sup>7</sup> *The Mechanism of Mendelian Heredity* (Morgan, Sturtevant, Muller, Bridges), 1915.

separate chromosomes at corresponding loci at each successive mitosis. The reduction from the diploid to the haploid number, according to the more generally accepted interpretation of the appearances during the meiotic phase, is due to the adhering together in pairs of homologous chromosomes, each member of the set originally received from one parent lying alongside and in close contact with its mate received from the other. Later these bivalent chromosomes are resolved into their components so that the two groups destined one for either pole consist of whole dissimilar chromosomes, which then proceed to divide again longitudinally to furnish equivalent half chromosomes to each of the daughter nuclei. According to the view of Farmer the homologous chromosomes do not lie alongside, but become joined end to end. The longitudinal split seen in the bivalent structure is interpreted as a separation not of whole chromosomes but of half chromosomes already formed in anticipation of the second division of the meiotic phase. As however on either interpretation the same result is ultimately secured, viz.: the distribution of whole paternal and maternal chromosomes to different nuclei which now contain the haploid number, it is not essential to our present purpose to discuss the cytological evidence in support of these opposing views in further detail. Nor, indeed, would it be practicable within the limits of this Address. The obvious close parallel between the behaviour of the chromosomes—their pairing and separation—and that of Mendelian allelomorphs which similarly show pairing and segregation, first led to the suggestion that the factors controlling somatic characters are located in these structures. The ingenious extension of this view which has been elaborated by Morgan and his co-workers presumes the arrangement of the factors in linear series after the manner of the visible chromomeres—the beadlike elements which can be seen in many organisms to compose the chromatin structure—each factor and its opposite occupying corresponding loci in homologous chromosomes. From this conception follows the important corollary of the segregation of the factors during the process of formation and subsequent resolution of the bivalent chromosomes formed at the reduction division. We should suppose, according to Morgan, in the case of characters showing *independent* inheritance and giving identical Mendelian ratios whichever way the mating is made, and however the factorial combination is brought about, that the factors controlling the several characters are located in *different* chromosomes. Thus, in the case of *Datura* already mentioned, the two factors affecting sap colour and prickliness respectively would be presumed to be located so far apart in the resting chromatin thread that when separation into chromosomes takes place they become distributed to different members. Where unrelated characters show a *linked* inheritance the factors concerned are held on the other hand to lie so near together that they are always located in one and the same chromosome. Furthermore, and here we come to the most debatable of the assumptions in Morgan's theory, when the bivalent chromosome composed of a maternal and a paternal component gives rise at the reduction division to two single dissimilar chromosomes, these new chromosomes do not always represent the original intact maternal and paternal

components. It has been observed in many forms that the bivalent structure has the appearance of a twisted double thread. Already in 1909 cytological study of the salamander had led Janssen<sup>8</sup> to conclude that fusion might take place at the crossing points, so that when the twin members ultimately draw apart each is composed of alternate portions of the original pair. Morgan explains the breeding results obtained with *Drosophila* by a somewhat similar hypothesis. He also conceives that in the process of separation of the twin lengths of chromatin cleavage between the two is not always clean, portions of the one becoming interchanged with corresponding segments of the other, so that both daughter chromosomes are made up of complementary sections of the maternal and paternal members of the duplex chromosome. To picture this let us imagine that two bars of that delectable substance, Turkish Delight, one pink and one white, are laid alongside and are then given a half twist round each other and pressed together. If, with a knife inserted between the two pieces at one end, the double bar is now sliced longitudinally down the middle neither of the two halves will be wholly pink or wholly white. Each half will be particoloured, the pink portion in one and the portion which is white in the other representing corresponding regions of the original bars. If the complete twist is made, or if the number of turns is still further increased before the slicing, the number of alternately coloured portions will naturally be increased correspondingly. Though the precise manner in which the postulated chromosomal interchange is brought about in Janssen's 'chiasmatype' and Morgan's 'crossing-over' scheme is different, the resulting gametic output would be the same. A critical examination by Wilson and Morgan,<sup>9</sup> from different aspects, of Janssen's interpretation of the cytological evidence including discussion of his latest suggestion that in the case of compound ring chromosomes cleavage in one plane would result in the separation of homologous elements in one ring but not in another has just appeared. These authors are not disposed to accept Janssen's conclusions,<sup>10</sup> but reserve their final statement pending the appearance of his promised further contribution. Should Janssen's view of the evolutions of these complex chromosome-structures be upheld, the process of segregation might in such cases become extended over more than one mitosis, as on the reduplication theory is conceived to be the case at some point, though evidence in this direction has hitherto been lacking. Bisection of a bivalent chromosome in this fashion might, moreover, yield the class of results to explain which Morgan has found it necessary to have recourse to hypothetical lethal factors. On the main issue, however, both schemes are in accord. A physical basis for the phenomenon of linkage is found in the presumed nature and behaviour of the chromosomes, viz.: their colloidal consistency, their adhesion after pairing at the points of contact, when in the twisted condition, and their consequent failure to separate cleanly before undergoing the succeeding division.

<sup>8</sup> *La Cellule*, xxv.

<sup>9</sup> *Am. Nat.*, vol. 54, 1920.

<sup>10</sup> See *Comptes Rendus Soc. Belg. Biol.*, 1919.

According to Morgan the frequency of separation of linked characters is a measure of the distance apart in the chromosome of the loci for the factors concerned, and it becomes possible to map their position in the chromosome relatively to one another. In this attempt to find in cytological happenings a basis for the observed facts of inheritance our conception of the material unit in the sorting-out process has been pushed beyond the germ cell and even the entire chromosome to the component sections and particles of the latter structure.

To substantiate the 'chromosome' view the primary requisite was to obtain proof that a particular character is associated with a particular chromosome. With this object in view it was sought to discover a type in which individual chromosomes could be identified. Several observers working on different animals found that a particular chromosome differing in form from the rest could be traced at the maturation division, and that this chromosome was always associated with the sex character in the following manner. The female possessed an even number of chromosomes so that each egg received an identical number, including this particular sex-chromosome. The male contained an uneven number, having one fewer than the female, with the result that half the sperms received the same number as the egg including the sex-chromosome, and half were deficient in this particular chromosome. Eggs fertilised with sperms containing the full number of chromosomes developed into females, while those fertilised with sperms lacking this distinctive chromosome produced males. Morgan made the further discovery in the fruit fly *Drosophila ampelophila* that certain factors controlling various somatic characters were located in the sex-chromosome. The inheritance of these characters and of sex evidently went together. A male exhibiting the dominant condition of such a sex-linked character bred to a recessive female gave daughters all dominant and sons all recessive (fig. 2), but in the reciprocal cross both sons and daughters proved to be all dominants (fig. 1). Since the mother with the dominant factor contributed it to all her children (fig. 1), whereas, where the father bore it, it descended only to his daughters (fig. 2), it was apparent that the female was homozygous and the male heterozygous for the somatic character. Further, although no distinction is observable in this species between the sperms, the occurrence of this sex-linked form of inheritance indicated that here, as in the other cases mentioned, it is the female which behaves as a homozygote for the sex character and the male as a heterozygote, the sex-chromosomes of some sperms differing presumably in character, though not in appearance, from those of others. The sperms of *Drosophila* are therefore conceived as of two kinds, one containing the same sex-chromosome as the eggs, the so-called X chromosome, and the other a mate of a different nature, the Y chromosome, which appears to be inert and unable to carry the dominant allelomorphs. If, now, we suppose the factor for the sex-linked somatic character to be located in the X chromosomes we understand why the dominant female, which is XX, and therefore furnishes an X chromosome to every egg, should contribute the dominant character to all her

offspring. And conversely, why the dominant male, which is XY, when bred to a recessive female, produces offspring which are either female and dominant or male and recessive.

Tracing the chromosomes into the next ( $F_2$ ) generation we see also the reason for the different result obtained from the reciprocal matings if the  $F_1$  individuals are inbred. When the *female* parent has the dominant sex-linked character half the eggs of the daughters and half the sperms of the sons receive this character. As the sperms receive it along with the X chromosome fertilisation of either kind of egg by these X sperms will cause the character to descend to each grand-daughter. The grandsons, on the other hand, since they arise from fertilisation by the sperms lacking the dominant character—*i.e.*, by the Y sperms—will be dominant or recessive according as these sperms unite with the one type of egg or with the other. Thus we get the Mendelian  $F_2$  ratio 3D to 1R (fig. 1), but so linked with sex that the dominant class comprises half the males and all the females, while the remaining half of the males make up the recessive class. Where it is the *male* parent that carries the dominant, and where therefore the dominant character passes along with the X chromosome *only* to the daughters in  $F_1$ , their eggs, as in the reciprocal cross, are of two kinds, but the sons' sperms all carry the recessive allelomorphs. Both kinds of eggs being fertilised with both X sperms and Y sperms, the dominant and recessive characters will occur *equally in both sexes* among the grandchildren, and we get the Mendelian ratio of 1D to 1R (fig. 2). Muller<sup>11</sup> puts the number of factors already located in the X chromosome of *Drosophila* at not less than 500, and in those that have so far been investigated this form of inheritance has been found to hold.

Instances of sex-linked inheritance are now known in many animals, some of which are strictly comparable with *Drosophila*, others follow the same general principle, but have the relations of the sexes reversed, as exemplified by the moth *Abraxas*, which has been worked out by Doncaster,<sup>12</sup> whose sudden death we have so recently to deplore. Here the female is the heterozygous sex, and contains the dummy mate of the sex-chromosome.

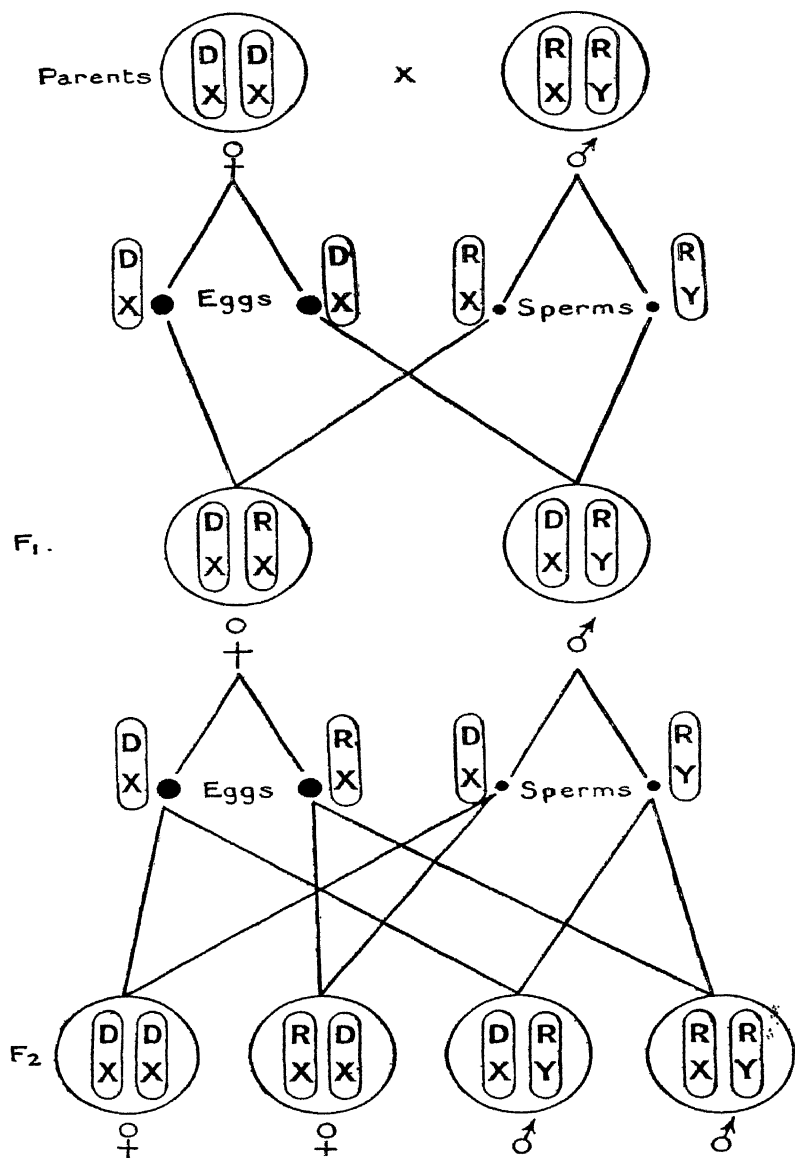
The behaviour of the sex-chromosomes as here outlined suffices to account for the occurrence of sex-linked inheritance, but the relations found to hold between one sex-linked character and another need further explanation. If a cross is made involving two sex-linked characters, the  $F_1$  females when tested by a double recessive male are found to produce the expected four classes of gametes, but not in equal proportions, nor in the same proportions in the case of different pairs of sex-linked characters. Partial linkage (coupling) occurs of the kind which has already been described for the Stock and the Sweet Pea. The parental combinations predominate, the recombinations ('cross-overs') comprise the smaller categories. The strength of the linkage varies, however, for different characters, but is found to be constant for any given pair. Since the sex-linked factors are by hypothesis

<sup>11</sup> *Am. Nat.*, vol. liv., 1920.

<sup>12</sup> *Rep. Evolution Committee*, iv., 1908.

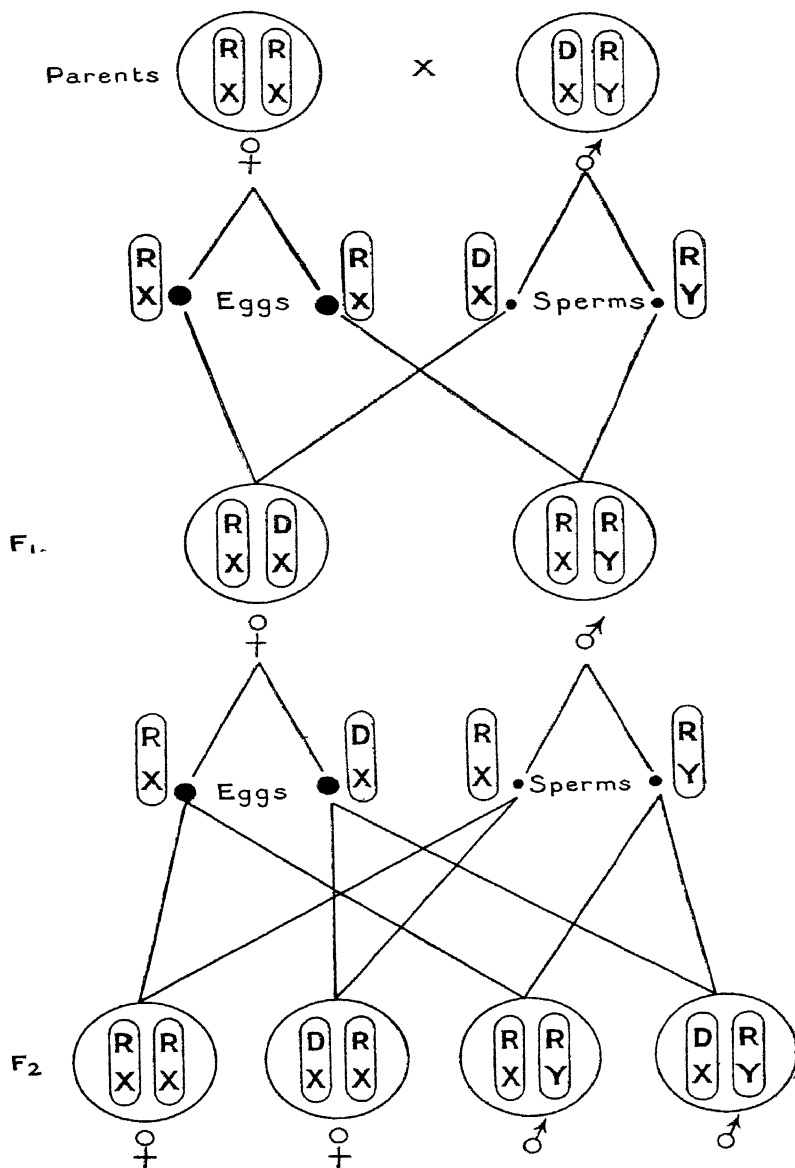


Fig. 1



3 D : 1 R

Fig 2



1 D : 1 R.

carried in the sex-chromosomes, a clean separation of homologous members at meiosis should result in the characters which were associated in the parents remaining strictly in the same combination in each succeeding generation. The fact that this is not the case has led Morgan to conclude that an interchange of chromosome material must take place at this phase among a proportion of the gametes, and that the percentage of these 'cross-overs' will depend on the distance apart of the loci of the factors concerned. This phenomenon of linkage may also be exhibited by pairs of characters which show *no* sex-linkage in their inheritance. The factors involved in these latter cases must presumably, therefore, be disposed in one of the chromosomes which is not the sex-chromosome.

To this brief sketch of the main points of Morgan's chromosome theory must be added mention of the extremely interesting relation which lends strong support to his view, and the significance of which seems scarcely to admit of question, viz.: that in *Drosophila ampelophila* there are four pairs of chromosomes, and that the linkage relations of the hundred and more characters investigated indicate that they form four distinct groups. It is hardly possible to suppose that the one fact is not directly connected with the other. The interesting discovery of Bridges<sup>13</sup> that the appearance of certain unexpected categories among *Drosophila* offspring, where females of a particular strain were used, coincided with the presence in these females of an additional chromosome adds another link in the chain of evidence. On examination it was found that in these females the X chromosome pair occasionally failed to separate at the reduction division, and consequently that the two XX chromosomes sometimes both remained in the egg, and sometimes both passed out into the polar body. Hence there arose from fertilisation of the XX eggs some individuals containing *three* sex-chromosomes, with the resulting upset of the expectation in regard to sex-limitation of characters which was observed.

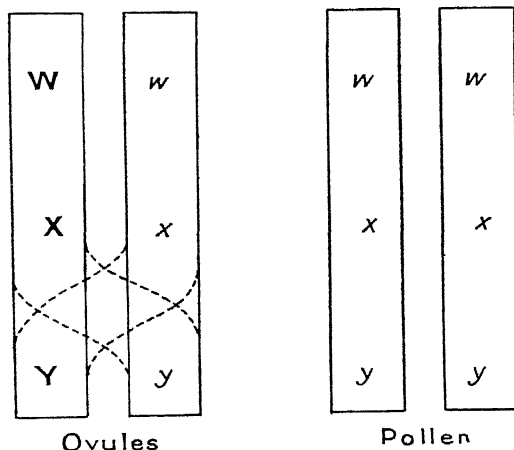
It, however, remains a curious anomaly that in the cross-bred *Drosophila* male no corresponding crossing-over of linked characters, whether associated with the sex character or not, has yet been observed. His gametes carry only the same factorial combinations which he received from his parents. For this contrast in the behaviour between the sexes there is at present no explanation. The reverse condition has been described by Tanaka<sup>14</sup> in the silkworm. Here interchange takes place in the male but not in the female.

It must then be acknowledged that Morgan's interpretation of the cytological evidence has much in its favour. The striking parallel between the behaviour of the chromosomes and the distributional relations of Mendelian allelomorphs is obvious. The existence in *Drosophila ampelophila* of four pairs of chromosomes and of four sets of linked characters can hardly be mere coincidence. The employment of the smaller physical unit in accounting for the reshuffling of characters in their transmission commends itself in principle. The necessity for postulating the occurrence of some orderly irregularity in the hereditary

<sup>13</sup> *J. Exp. Zool.*, xv., 1913.

<sup>14</sup> *J. Coll. Agr.*, Sapporo, Japan, 1913-14.

process in order to explain the phenomenon of partial linkage is, it will be seen, inherent alike in both theories. When, however, we come to examine the general applicability of Morgan's theory we are confronted with a considerable body of facts among plants which we find difficult to reconcile with the requirement that factorial segregation is accomplished by means of the reduction division. An instance in which this is particularly clearly indicated is that of the sulphur-white Stock. I have chosen this example because here we have to do with two characters which are distinguished with the utmost sharpness, viz.: plastid colour and flower form. The peculiar behaviour of this strain is due to the fact that not only are the two factors for flower form (singleness and doubleness) differently distributed to the male and female sides of the individual, as in all double-throwing Stocks, but the factor controlling plastid colour likewise shows linkage with the sex nature of the germ cells. As a result every individual, even though self-fertilised, yields a mixed offspring, consisting chiefly of single whites and double creams, but including a small percentage of double whites. So far as the ovules are concerned, the mode of inheritance can be accounted for on either theory. According to the reduplication hypothesis the factors  $X Y^{15}$  producing singleness and  $W$  giving white plastids are partially coupled so as to give the gametic ratio on the female side  $7 WXY : 1 WXY : 1 wxY : 7 wxy$ .<sup>16</sup> On the chromosome scheme the factorial group  $WXY$  must be assumed to be disposed in one member of the bivalent chromosome formed at meiosis, the corresponding recessive allelomorphs  $wxy$  in the other. If the three factors be supposed to be arranged in the chromosome in alphabetical order, and if, on separation, a break takes place between the loci of the two factors for flower form (as shown), so as to give 'cross-overs' of  $Y$



<sup>15</sup> The letters  $X$  and  $Y$  are used here to denote particular factors, not, as in Morgan's scheme, the entire sex-chromosomes.

<sup>16</sup> Or possibly  $15 : 1 : 1 : 15$ .

and y in about 12 per cent. of the gametes, the occurrence of such 'cross-overs' would fulfil the required conditions. But the case of the pollen presents a distinct difficulty on this latter view. This Stock is distinguished both from the *Drosophila* and the *Abraxus* type by the fact that none of the male germs carry either of the dominant characters. In place of the XX—XY form of sex-linked inheritance in the former type and the WZ—ZZ in the latter, we should need to regard this form as constituting a new class, which we might represent as DR—RR, thus indicating that both members of the bivalent chromosome on the male side appear to be inert and able to carry only the recessive characters, and hence are represented as RR, in contrast with the DR pair of the female side. By this formula we can indicate the behaviour of the several double-throwing strains. It is, besides, becoming clear, I think, from recent results that there is no 'crossing over' of these factors on the male side in the F<sub>1</sub> cross-breeds. But the real difficulty is to explain why these factors are confined to the female side in the ever-sporting individual. This may result from aberrant behaviour or loss of chromosomes at some point in pollen development. On this point I hope that evidence will shortly be available. Failing such evidence the presumption is that the elimination of XY (and in one strain of W) must have taken place *prior to*, and not *at*, the moment of the maturation division. Morgan's proposal to fit the pollen into his scheme for *Drosophila* by having recourse to hypothetical lethal factors does not appeal to the observer, who finds the pollen all uniformly good and every ovule set. Zygotic lethals are clearly not in question under these circumstances. The supposition of gametic lethals confined to the pollen appears far-fetched, seeing that of the missing combinations two, viz.: single white, the double dominant, and double white a dominant-recessive, occur in the ovules, and the third, the single cream, the other dominant-recessive, exists as a pure strain, so that the homozygous condition is evidently not in itself a cause of non-development. Other examples suggesting premeiotic segregation can be quoted, notably cases among variegated plants and plants showing bud sports, where somatic segregation appears to be of regular occurrence. Among the Musciniae the present evidence appears to show that the sex potentiality segregates in some forms at the division of the spore mother cells, so that already the spores possess a sex character; while in other species this separation takes place later, during the development of the gametophyte, the spores being then all alike and undifferentiated in this respect. In *Funaria hygrometrica*, an example of the latter class, an attempt has been made by E. J. Collins<sup>17</sup> to ascertain the stage at which sex segregation takes place by inducing the growth of new individuals from isolated portions of the vegetative tissues of the gametophyte. No doubt when the evidence is derived from experiments in which a portion of the plant has been severed from the rest, it is possible to urge that the result obtained is not necessarily indicative of the potentiality in the intact organism. Phenotypic appearance is the product of a reaction system, in which the internal as well as the external environment plays its part. We

have, for example, evidence that the manifestation of a character may be dependent upon the variation of internal conditions with age; in other words, a time relation may be involved.<sup>18</sup> Or, again, upon the state of general internal equilibrium resulting from the relation of one morphological member or region to another. Thus removal of the lamina of the leaf, so as to leave only the midrib, may cause the mutilated individual to develop hairs on the stems and petioles in the same environment in which the intact individual remains hairless. Injury from attack by insects in a glabrous form may in like manner lead to the production of hairs which, by their resemblance to those of an allied species, show that the pathological condition set up has caused genetic potentiality to become actual. But even if we exclude the class of evidence to which objection on these grounds might be made, there still remain various cases of normal types, where, unless the behaviour of the chromosomes should point to a different explanation, it seems most natural to assume that segregation takes place before the reduction division.

It has been argued from time to time that any scheme representing the mechanism of Heredity which leaves out of account the cytoplasm must prove inadequate. This general statement has been expressed in more definite form by Loeb,<sup>19</sup> who holds that the egg cytoplasm is to be looked upon as determining the broad outlines, in fact as standing for the embryo 'in the rough,' upon which are impressed in the course of development the characteristics controlled by the factors segregated in the chromosomes. The arguments in favour of the view that the cytoplasm, apart from its general functions in connection with growth and nutrition, is the seat of a particular hereditary process are mainly derived from observation upon embryonic characters in certain animals, chiefly Echinoderms, where the inheritance appears to be purely maternal. It has been shown, however, that such female prepotency is no indication that inheritance of the determining factors takes place through the cytoplasm. Other causes may lead to this result. It has been observed, for example, that hybrid sea-urchin larvae, which at one season of the year were maternal in type, at another were all paternal in character, showing that the result was due to some effect of the environment. Again, where the hybrid plutei showed purely maternal characters it was discovered by Baltzer<sup>20</sup> that in the earliest mitoses of the cross-fertilised eggs a certain number of chromosomes fail to reach the poles, and are consequently left out of the daughter nuclei. The chromosomes thus lost probably represent those contributed by the male gamete, for in both parents certain individual chromosomes can be identified owing to differences in shape and size. After this process of elimination those characteristic of the male parent could not be traced, whereas the one pair distinctive of the female parent was still recognisable. In the reciprocal cross where the first mitosis

<sup>18</sup> As in the case of characters which exhibit a regular change of phase, e.g., the colour of white and cream Stocks is indistinguishable in the bud, and a yellow-seeded Pea is green at an earlier stage.

<sup>19</sup> *The Organism as a Whole*, 1916.

<sup>20</sup> *Archiv für Zellforschung*, v., 1910.

follows a normal course the embryos are intermediate in regard to character of the skeleton, thus affording proof of the influence of the male parent. Another type of case is found in the silkworm. Here a certain rate character determining the time of hatching out of the eggs has been shown to exhibit normal Mendelian inheritance, the appearance that it is transmissible by the female through the cytoplasm alone being delusive. The eggs are always laid in the spring. According as they hatch out immediately so that a second brood is obtained in the year, or do not hatch out for twelve months, the female parent laying the eggs is described as bivoltin or univoltin. Now the length of interval before hatching is obviously an egg character, and therefore maternal in origin. Consequently when a cross is made between a univoltin female and a bivoltin male the eggs laid are not cross-bred in respect of this character, any more than the seed formed as a result of a cross is cross-bred in respect of its seed coat, which is a maternal structure. The silkworm mother being univoltin, the eggs will not hatch out until the following spring. The  $F_1$  mother will in turn lay eggs which again take twelve months to hatch, since the long-period factor is the dominant. It is not until the eggs of the  $F_2$  generation are laid that we see the expression of the character introduced by the univoltin father. For some of the egg batches hatch at once, others not for twelve months, showing that of the  $F_2$  females some were uni- and some bi-voltin, and hence that the egg character in any generation depends upon both the maternal and the paternal antecedents of the female producing the eggs. Consequently, in the case of an egg character the effects of inheritance must be looked for in the generation succeeding that in which the somatic characteristics of the zygote become revealed. We find in fact that in almost all instances where the evidence is suggestive of purely cytoplasmic inheritance, fuller investigation has shown that the explanation is to be found in one of the causes here indicated. The case of some plants where it has been established that reciprocal hybrids are dissimilar still, however, remains to be cleared up. Among such may be cited certain *Digitalis* hybrids. Differences in the reciprocal hybrids of *D. grandiflora* and *D. lutea* were described by Gaertner, and in the earlier literature dealing with *Digitalis* species hybrids other cases are to be found. In more recent years J. H. Wilson<sup>21</sup> has repeated the crossing of *D. purpurea* and *D. lutea*, and states that the reciprocals are indistinguishable during the vegetative period, but that they differ in size and colouring of the flowers, the resemblance being the greater in each case to the seed parent. A detailed comparison of the differential characters of the reciprocal hybrids of *D. purpurea* and *D. grandiflora* has been set out by Neilson Jones,<sup>22</sup> who similarly finds in both matings a greater resemblance to the mother species. We know nothing as yet of the cytology of these cases, and it is not improbable that the interpretation may be found in some aberrant behaviour of the chromosomes. An instance in a plant type where a definite connection appears traceable between chromosome behaviour and somatic appearance has been

<sup>21</sup> *Rep. Third International Congress on Genetics*, R.H.S. 1906.

<sup>22</sup> *J. of Genetics*, vol. ii., 1912.

recently emphasised by Gates,<sup>23</sup> who attributes the peculiarity of the *lata* mutation in *Oenothera* (which has arisen as a modification at different times from each of three distinct species) to an irregularity in meiosis in the germ mother cells whereby one daughter cell receives an extra duplicate chromosome while the sister cell lacks this chromosome. The cell with the extra chromosome fertilised by a normal germ produces a *lata* individual. On the chromosome view every normal fertilised egg contains a double set of chromosomes, each carrying a complete set of the factor elements. Hence, if some of the one set become eliminated we can still imagine that a normal though undersized individual might develop. The converse relation where increased size goes with multiplication of chromosomes was discovered by Gregory,<sup>24</sup> in a *Primula*, and occurs also in *Oenothera gigas*, a mutant derived from *O. Lamarckiana*. Gregory found in his cultures giant individuals which behaved as though four instead of two sets of factors were present, and upon examination these individuals were found to contain twice the normal number of chromosomes. It is interesting in this connection to recall the results obtained by Nemec<sup>25</sup> as the result of subjecting the root tips of various plants to the narcotising action of chloral hydrate. Under this treatment cells undergoing division at the time were able to form the daughter nuclei, but the production of a new cell wall was inhibited. The cells thus became binucleate. If on recovery these cells were to fuse before proceeding to divide afresh a genuine tetraploid condition would result. So few cases of natural tetraploidy have so far been observed that we have as yet no clue to the cause which leads to this condition.

The conclusions to which we are led by the considerations which have here been put forward are, in the main, that we have no warrant in the evidence so far available for attributing special hereditary processes to the cytoplasm as distinct from the nucleus. On the other hand, there is a very large body of facts pointing to a direct connection between phenotypic appearance and chromosomal behaviour. In animals the evidence that the chromosomes constitute the distributional mechanism may be looked upon as almost tantamount to proof; in plants the observations on *Drosera*, *Primula*, *Oenothera*, *Sphaerocarpus* are in harmony with this view. When we come, however, to the question of linkage and general applicability of the conception of 'crossing over' as adopted by Morgan and his school we are on less certain ground. In *Drosophila* itself, the case which the scheme was framed to fit, the entire absence of 'crossing over' in the male remains unaccounted for, while the evidence from certain plant types appears to be definitely at variance with one of its fundamental premises. If segregation at the recognised reduction division is definitely established for animal types, then we must conclude that the sorting-out process may follow a different course in the plant.

The question as to what is the precise nature of the differences for

<sup>23</sup> *Nerv. Phytologist*, vol. xix., 1920.

<sup>24</sup> *Proc. Roy. Soc.*, vol. lxxxvii. B, 1914.

<sup>25</sup> *Jahrb. f. wiss. Bot.*, xxxix., 1904. 'Das Problem der Begrüchtungsvorgänge,' 1910.



which the Mendelian factors stand is constantly before the mind of the breeder, but we are only now on the threshold of investigation in this direction, and it is doubtful whether we can as yet give a certain answer in any single instance. Still less are we able to say what the actual elements or units which undergo segregation may be. In the case of such allelomorphic pairs as purple and red sap colour or white or cream plastid colour it may be that the difference is *wholly qualitative*, consisting merely in the formation or non-formation of some one chemical substance. But the majority of characteristics are not of this hard-and-fast type. Between some the distinction appears to be one of *range*—to be quantitative rather than, or as well as, qualitative in nature, and range must mean, presumably, either cumulative effect or a force or rate difference. It may well be, for example, that with some change in physiological equilibrium accompanying growth and development, factorial action may be enhanced or accelerated, or, on the other hand, retarded or even inhibited altogether, and a regional grading result in consequence. Range in a character is not confined to, though a common characteristic of, individuals of cross-bred origin. It may be a specific feature, both constant and definite in nature. For example, a change as development proceeds from a glabrous or nearly glabrous to a hairy condition is not of unusual occurrence in plants. In the Stock such a gradational assumption of hairiness is apparent no less in the homozygous form containing a certain weak allelomorph controlling surface character, when present with the factors for sap colour, than in those heterozygous for this or some other essential component. We see a similar transition in several members of the *Scrophulariaceæ*—e.g., in various species of *Digitalis*, in *Antirrhinum majus*, *Antirrhinum Orontium*, *Anarrhinum pedatum*, *Pentstemon*, and *Nemesia*. In perennials an annual recurrence of this change of phase may be seen, as in various species of *Viola* and in *Spiræa Ulmaria*. It is somewhat curious that the transition may be in the same direction—from smoothness to hairiness—in forms in which the dominant-recessive relation of the two conditions is opposite in nature, as in *Matthiola* on the one hand and *Digitalis purpurea nudicaulis* on the other. Manifestation of the dominant characteristic gradually declines in the Fox-glove, while it becomes more pronounced in the Stock. In some, perhaps all, of these cases the allelomorphs may stand for certain states of physiological equilibrium, or such states may be an accompanying feature of factorial action. A change of phase may mean an altered balance, a difference of rhythm in interdependent physiological processes. In the case, for instance, of a certain sub-glabrous strain of Stock in which the presence of a single characteristically branched hair or hair-tuft over the water-gland terminating the midrib in a leaf otherwise glabrous is an hereditary character, it is hardly conceivable that there is a localisation in this region of a special hair-forming substance. It seems more probable that some physiological condition intimately connected with the condition of water-content at some critical period is a causal factor in hair production, and that this condition is set up over the whole leaf in the type, but in the particular strain in question is maintained only at the point which receives the

largest and most direct supply. In this same strain a leaf may now and again be found lacking this hydathode trichome in an otherwise continuous hair-forming series, an occurrence which may well result from a slight fluctuation in physiological equilibrium such as is inherent in all vital processes—a fluctuation which, when the genetic indicator is set so near to the zero point, may well send it off the scale altogether. If, as is not improbable in this and similar cases, we are concerned with a complex chain of physiological processes, investigation of the nature of the differences for which the allelomorphs stand may present a more difficult problem than where the production of a particular chemical compound appears to be involved. In such a physiological conception we have probably the explanation of the non-appearance of the recessive character in certain dominant cross-breds.

Up to this point we have treated of the organism from the aspect of its being a wholly self-controlled, independent system. As regards some characteristics, this may be regarded as substantially the case. That is to say, the soma reflects under all observed conditions the genetic constitution expressed in the Mendelian formula. Correspondence is precise between genotypic potentiality and phenotypic reality, and we have so far solved our problem that we can predict certainly and accurately the appearance of offspring, knowing the constitution of the parents. In such cases we may say that the efficiency of the genetic machine works out at 100 per cent., the influence of external environment at 0. Our equation  $\text{somatic appearance} = \text{factorial constitution}$  requires no correction for effect of conditions of temperature, humidity, illumination, and the like. But most somatic characters show some degree of variability. Phenotypic appearance is the outcome primarily of genotypic constitution, but upon this are superposed fluctuations, slight or more pronounced, arising as the result of reaction to environmental conditions. In the extreme case the genetic machinery may, so to speak, be put out of action; genotypic potentiality no longer becomes actual. We say that the character is not inherited. We meet with such an example in *Ranunculus aquatilis*. According to Mer,<sup>26</sup> the terrestrial form of this plant has no hairs on the ends of the leaf segments, but in the aquatic individual the segments end in needle-shaped hairs. That is to say, hairs of a definite form are produced in a definite region. Again, Massart<sup>27</sup> finds that in *Polygonum amphibium* the shoot produces characteristic multicellular hairs when exposed to the air, but if submerged it ceases to form them on the new growth. Every individual, however bred, behaves in the same manner, and must therefore have the same genetic constitution. In an atmospheric environment genotypic expression is achieved, in water it becomes physiologically impossible. A limitation to genotypic expression may in like manner be brought about by the internal environment, for the relation of the soma to the germ elements may be looked upon in this light. Thus in the case of a long-pollened and round-pollened Sweet Pea Bateson and Punnett<sup>28</sup> found that the

<sup>26</sup> *Bull. Soc. Bot. de France*, i. 27, 1880.

<sup>27</sup> *Bull. Jard. Bot. Bruxelles*, i. 2, 1902.

<sup>28</sup> *Report to the Evolution Committee*, Roy. Soc., ii., 1905.

F<sub>1</sub> pollen grains are all long, yet half of them carry the factor for roundness. If we take the chromosome view, and if it be presumed that the factor for roundness is not segregated until the reduction division, the cytoplasm of the pollen mother cells may be supposed to act as a foreign medium owing to a mixture of qualities having been impressed upon it through the presence of the two opposite allelomorphs before the moment of segregation. We should consequently infer that the round pollen shape is only produced when the round-factor-bearing chromosome is surrounded by the cytoplasm of an individual which does not contain the long factor. If, further, we regard the result in this case as indicative of the normal inter-relation of nucleus and cytoplasm in the hereditary process, we shall be led to the view that whatever the earlier condition of mutual equilibrium or interchange between these two essential cell constituents may be, an ultimate stage is reached in which the rôle of determining agent must be assigned to the nucleus. To pursue this theme farther, however, in the present state of our knowledge would serve no useful purpose.

Before bringing this Address to a conclusion I may be permitted to add one word of explanation and appeal. In my remarks I have deliberately left on one side all reference to the immense practical value of breeding experiments on Mendelian lines. To have done so adequately would have absorbed the whole time at my disposal. It is unnecessary to-day to point out the enormous social and economic gain following from the application of Mendelian methods of investigation and of the discoveries which have resulted therefrom during the last twenty years, whether we have in mind the advance in our knowledge of the inheritance of ordinary somatic characters and of certain pathological conditions in man, of immunity from disease in races of some of our most important food plants, or of egg-production in our domestic breeds of fowls.

My appeal is for more organised co-operation in the experimental study of Genetics. It is a not uncommon attitude to look upon the subject of Genetics as a science apart. But the complex nature of the problems confronting us requires that the attacking force should be a composite one, representing all arms. Only the outworks of the fortress can fall to the vanguard of breeders. Their part done, they wait ready to hand over to the cytologists with whom it lies to consolidate the position and render our foothold secure. This accomplished, the way is cleared for the main assault. To push this home we urgently need reinforcements. It is to the physiologists and to the chemists that we look to crown the victory. By their co-operation alone can we hope to win inside the citadel and fathom the meaning of those activities which take shape daily before our eyes as we stand without and observe, but the secret of which is withheld from our gaze.

# British Association for the Advancement of Science.

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SECTION L: CARDIFF, 1920.

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## ADDRESS TO THE EDUCATIONAL SCIENCE SECTION

BY  
SIR ROBERT BLAIR, LL.D.,  
PRESIDENT OF THE SECTION.

### *Introduction.*

THE requirements of the Act of 1918 and the endeavour to frame scales of salaries for teachers on a national basis are, at present, absorbing so much of the energy of those engaged in educational administration that I have thought it advisable to turn our attention from the immediate needs of the day to two of the wider aspects of our educational activities, which belong to the spirit rather than to the form of our educational system.

It is natural that in this meeting of the British Association for the Advancement of Science I should take first the Science of Education.

### I.

The value to education of science and the scientific method has hitherto been for the most part indirect and incidental. It has consisted very largely in deductions from another branch of study, namely, psychology, and has resulted for the most part from the invasion into education of those who were not themselves educationists. A moment has now been reached when education itself should be made the subject of a distinct department of science, when teachers themselves should become scientists.

There is in this respect a close analogy between education and medicine. Training the mind implies a knowledge of the mind, just as healing the body implies a knowledge of the body. Thus, logically, education is based upon psychology, as medicine is based on anatomy and physiology. And there the text-books of educational method usually content to leave it. But medicine is much more than applied physiology. It constitutes an independent system of facts, gathered and analysed, not by physiologists in the laboratory, but by physicians working in the hospital or by the bedside. In the same way, then, education as a science should be something more than mere applied psychology. It must be built up not out of the speculations of theorists, or from the deductions of psychologists, but by direct, definite, *ad hoc* inquiries concentrated upon the problems of the class-

room by teachers themselves. When by their own researches teachers have demonstrated that their art is, in fact, a science, then, and not till then, will the public allow them the moral, social, and economic status which it already accords to other professions. The engineer and the doctor are duly recognised as scientific experts. The educationist should see to it that his science also becomes recognised, no longer as a general topic upon which any cultured layman may dogmatise, but as a technical branch of science, in which the educationist alone, in virtue of his special knowledge, his special training, his special experience, is the acknowledged expert.

Educational science has hitherto followed two main lines of investigation: first, the evaluation and improvement of teachers' methods; secondly, the diagnosis and treatment of children's individual capacities.

### *I. The Psychology of the Individual Child.*

It is upon the latter problem, or group of problems, that experimental work has in the past been chiefly directed, and in the immediate future is likely to be concentrated with the most fruitful results. The recent advances in 'individual psychology'—the youngest branch of that infant science—have greatly emphasised the need, and assisted the development, of individual teaching. The keynote of successful instruction is to adapt that instruction to the individual child. But before instruction can be so adapted, the needs and the capacities of the individual child must first be discovered.

#### *A. Diagnosis.*

Such discovery (as in all sciences) may proceed by two methods, by observation and by experiment.

(1) The former method is in education the older. At one time, in the hands of Stanley Hall and his followers—the pioneers of the Child-Study movement—observation yielded fruitful results. And it is perhaps to be regretted that of late simple observation and description have been neglected for the more ambitious method of experimental tests. There is much that a vigilant teacher can do without using any special apparatus and without conducting any special experiment. Conscientious records of the behaviour and responses of individual children, accurately described without any admixture of inference or hypothesis, would lay broad foundations upon which subsequent investigators could build. The study of children's temperament and character, for example—factors which have not yet been accorded their due weight in education—must for the present proceed upon these simpler lines.

(2) With experimental tests the progress made during the last decade has been enormous. The intelligence scale devised by Binet for the diagnosis of mental deficiency, the mental tests employed by the American army, the vocational tests now coming into use for the selection of employees—these have done much to familiarise, not school teachers and school doctors only, but also the general public, with the aims and possibilities of psychological measurement. More recently an endeavour has been made to assess directly the results of school

instruction, and to record in quantitative terms the course of progress from year to year, by means of standardised tests for educational attainments. In this country research committees of the British Association and of the Child-Study Society have already commenced the standardisation of normal performances in such subjects as reading and arithmetic. In America attempts have been made to standardise even more elusive subjects, such as drawing, handwork, English composition, and the subjects of the curriculum of the secondary school.

### B. *Treatment.*

This work of diagnosis has done much to foster individual and differential teaching—the adaptation of education to individual children, or at least to special groups and types. It has not only assisted the machinery of segregation—of selecting the mentally deficient child at one end of the scale and the scholarship child at the other end; but it has also provided a method for assessing the results of different teaching methods as applied to these segregated groups. Progress has been most pronounced in the case of the sub-normal. The mentally defective are now taught in special schools, and receive an instruction of a specially adapted type. Some advance has more recently been made in differentiating the various grades and kinds of so-called deficiency; and in discriminating between the deficient and the merely backward and dull. With regard to the morally defective and delinquent little scientific work has been attempted in this country, with the sole exception of the new experiment initiated by the Birmingham justices. In the United States some twenty centres or clinics have been established for the psychological examination of exceptional children; and in England school medical officers and others have urged the need for ‘intermediate’ classes or schools not only to accommodate backward and borderline cases and cases of limited or special defect (*e.g.*, ‘number-defect’ and so-called ‘word-blindness’) but also to act as clearing-houses.

In Germany and elsewhere special interest has been aroused in super-normal children. The few investigations already made show clearly that additional attention, expenditure, study, and provision will yield for the community a far richer return in the case of the super-normal than in the sub-normal.

At Harvard and elsewhere psychologists have for some time been elaborating psychological tests to select those who are best fitted for different types of vocation. The investigation is still only in its initial stages. But it is clear that if vocational guidance were based, in part at least, upon observations and records made at school, instead of being based upon the limited interests and knowledge of the child and his parents, then not only employers, but also employees, their work, and the community as a whole, would profit. A large proportion of the vast wastage involved in the current system of indiscriminate engagement on probation would be saved.

The influence of sex, social status, and race upon individual differences in educational abilities has been studied upon a small scale. The differences are marked: and differences in sex and social status, when better understood, might well be taken into account both in

diagnosing mental deficiency and in awarding scholarships. As a rule, however, those due to sex and race are smaller than is popularly supposed. How far these differences, and those associated with social status, are inborn and ineradicable, and how far they are due to differences in training and in tradition, can hardly be determined without a vast array of data.

## II. *Teaching Methods.*

The subjects taught and the methods of teaching have considerably changed during recent years. In the more progressive types of schools several broad tendencies may be discerned. All owe their acceptance in part to the results of scientific investigations.

(1) Far less emphasis is now laid upon the *disciplinary value of subjects*, and upon subjects whose value is almost solely disciplinary. Following in the steps of a series of American investigators, Winch and Sleight in this country have shown very clearly that practice in one kind of activity produces improvements in other kinds of activities, only under very limited and special conditions. The whole conception of transfer of training is thus changed, or (some maintain) destroyed; and the earlier notion of education as the strengthening, through exercise, of certain general faculties has consequently been revolutionised. There is a tendency to select subjects and methods of teaching rather for their material than their general value.

(2) Far less emphasis is now laid upon an advance according to strict *logical sequence* in teaching a given subject of the curriculum to children of successive ages. The steps and methods are being adapted rather to the natural capacities and interests of the child of each age. This genetic standpoint has received great help and encouragement from experimental psychology. Binet's own scale of intelligence was intended largely as a study in the mental development of the normal child. The developmental phases of particular characteristics (*e.g.*, children's ideals) and special characteristics of particular developmental phases (*e.g.*, adolescence) have been elaborately studied by Stanley Hall and his followers. Psychology, indeed, has done much to emphasise the importance of the post-pubertal period—the school-leaving age, and the years that follow. Such studies have an obvious bearing upon the curriculum and methods for our new continuation schools. But it is, perhaps, in the revolutionary changes in the teaching methods of the infants' schools, changes that are already profoundly influencing the methods of the senior department, that the influence of scientific study has been most strongly at work.

(3) Increasing emphasis is now being laid upon *mental and motor activities*. Early educational practice, like early psychology, was excessively intellectualistic. Recent child-study, however, has emphasised the importance of the motor and of the emotional aspects of the child's mental life. As a consequence, the theory and practice of education have assumed more of the pragmatic character which has characterised contemporary philosophy.

The progressive introduction of manual and practical subjects, both in and for themselves, and as aspects of other subjects, forms the most

notable instance of this tendency. The educational process is assumed to start, not from the child's sensations (as nineteenth-century theory was so apt to maintain), but rather from his motor reactions to certain perceptual objects—objects of vital importance to him and to his species under primitive conditions, and therefore appealing to certain instinctive impulses. Further, the child's activities in the school should be, not indeed identical with, but continuous with, the activities of his subsequent profession or trade. Upon these grounds handicraft should now find a place in every school curriculum. It will be inserted both for its own sake, and for the sake of its connections with other subjects, whether they be subjects of school life, of after life, or of human life generally.

(4) As a result of recent psychological work, more attention is now being paid to the *emotional, moral, and æsthetic* activities. This is a second instance of the same reaction from excessive intellectualism. Education in this country has ever claimed to form character as well as to impart knowledge. Formerly, this aim characterised the Public Schools rather than the public elementary schools. Recently, however, much has been done to infuse into the latter something of the spirit of the Public Schools. The principle of self-government, for example, has been applied with success not only in certain elementary schools, but also in several colonies for juvenile delinquents. And, in the latter case, its success has been attributed by the initiators directly to the fact that it is corollary of sound child-psychology.

Bearing closely upon the subject of moral and emotional training is the work of the psycho-analysts. Freud has shown that many forms of mental inefficiency in later life—both major (such as hysteria, neurosis, certain kinds of 'shell-shock,' &c.) and minor (such as lapses of memory, of action, slips of tongue and pen)—are traceable to the repression of emotional experiences in earlier life. The principles themselves may, perhaps, still be regarded as, in part, a matter of controversy. But the discoveries upon which they are based vividly illustrate the enormous importance of the natural instincts, interests, and activities, inherited by the child as part of his biological equipment; and, together with the work done by English psychologists such as Shand and McDougall upon the emotional basis of character, have already had a considerable influence upon educational theory in this country.

(5) Increasing emphasis is now being laid upon *freedom* for individual effort and initiative. Here, again, the corollaries drawn from the psycho-analytic doctrines as to the dangers of repression are most suggestive. Already a better understanding of child-nature has led to the substitution of 'internal' for 'external' discipline; and the pre-determined routine demanded of entire classes is giving way to the growing recognition of the educational value of spontaneous efforts initiated by the individual, alone or in social co-operation with his fellows.

In appealing for greater freedom still, the new psychology is in line with the more advanced educational experiments, such as the work done by Madame Montessori and the founders of the Little Commonwealth.

(6) The *hygiene and technique of mental work* is itself being based



upon scientific investigation. Of the numerous problems in the conditions and character of mental work generally, two deserve especial mention—fatigue, and the economy and technique of learning.

But of all the results of educational psychology, perhaps the most valuable is the slow but progressive inculcation of the whole teaching profession with a scientific spirit in their work, and a scientific attitude towards their pupils and their problems. Matter taught and teaching methods are no longer exclusively determined by mere tradition or mere opinion. They are being based more and more upon impartial observation, careful records, and statistical analysis—often assisted by laboratory technique—of the actual behaviour of individual children.

## II.

I turn now to the second aspect.

So much of our educational system is voluntary that it is often called a dual system. But in speaking of a dual system only the primary stage is, as a rule, in our minds. Yet to foreign students some parts of our higher education, *e.g.*, the Public Schools, appear as that which is most definitely English in character. The Public Schools, however, form no part of the system of public (*i.e.* of State and municipal) education and are not directly associated with it.

The reasons are fairly obvious. Many of the Public Schools are centuries old; our public system began but fifty years ago. The Act of 1870 gave us only public elementary schools. More than thirty years elapsed before we had the beginnings of a system of secondary schools. Even to-day, with the comprehensive Act of 1918, whose primary object is to establish a national system of education, the Public Schools, owing largely to the fact that the Act is administered by 318 Local Education Authorities, retain a 'non-local' character.

The Public Schools of England have no parallel. They have their defects and their critics; but they have had a paramount influence on the intellectual and social life of the country. They are admired less for the intellectual severity of the class-room than for their traditions, their form of self-government, and as training places of a generous spirit. In the past the Public Schools in the education of the aristocracy achieved a national purpose. They were the nurseries of English thought and action. Now that the predominant power in the State has passed to the nation as a whole, it would only be in keeping with their long-cherished traditions if the Public Schools were to seek a share in the education of democracy. Moreover, the problems of Local Education Authorities are of such absorbing interest that the professional spirit of the Public Schoolmaster must be longing to assist in their solution.

The two older Universities have had a history, and have borne a part in the national life, analogous to, but on a much larger scale than, the Public Schools. They also are 'non-local': they serve the Empire. The newer Universities are much more local in character. Yet as a whole it can hardly be said that they exercise an important influence on the work of the Local Education Authorities. I am not overlooking the fact that the Universities, like the Public Schools, play their own

part in providing the most advanced education; nor that they place their best at the disposal of the Local Education Authorities' scholars and contribute a part of the teaching staff. I am, however, to-day suggesting a closer association with Local Education Authorities, and of bringing to bear more immediately on local and public education the wealth of their long experience and the riches of their accumulated knowledge.

There is a third group of institutions which have had a large share in English education. I refer to the endowed Grammar Schools. Partly of choice, partly through stress of circumstances, many of these schools have joined forces with the Local Education Authorities. With the recent rapid growth in the cost of maintenance and with inadequacy of other sources of income they have received 'aid' from the Authority. Some have become municipal schools: others have undertaken to bear their share in local work, but have retained their individuality of character and independence of Government, to both of which they are passionately attached. All have contributed much to the general storehouse of ideas, and the local system has been enriched by the co-operation of forces of different origin, methods, and historical significance.

All three groups of institutions were founded by the few whose spirit in so far as it sought the spread of education has now passed to the multitude. They are all national institutions, but, with the exceptions to which I have referred, they form no part of the national system administered by Local Education Authorities and supervised by the Board of Education. I do not, of course, suggest control. That is obviously impossible in the case of two of the groups. Nor am I to-day thinking of making constructive proposals as to the forms of associations. Such proposals will, I hope, be put forward later in the week. For the moment it will be sufficient to add that the association desired is direct and close rather than indirect and remote, and in teaching rather than in administration.

There is one further group which I cannot pass in silence: the private schools. Each Local Education Authority must, under Section 1 of the Act of 1918, submit a scheme for the progressive and comprehensive organisation of education within its area. Presumably, each Local Education Authority will include the local 'places' in efficient private schools as part of the accommodation already provided in the area. All such efficient private schools, whether run for private profit or not, reduce the provision to be made by the Authority. To the extent to which they relieve the burden on the Authority they are therefore contributing to the public service. In return the Authority, while it cannot financially assist schools conducted for private profit, can confer advantages through close association with its organisation. All private schools doing local work, at all events all which claim to be efficient, would therefore serve their own interests and render public service by entering into communication with the Authority and getting the lines of local co-operation satisfactorily adjusted.

It would not be possible to exhaust the possibilities of co-operation of voluntary endeavour with the public system, even if my whole paper had been devoted to this subject. I am anxious, however, to carry my suggestions one step further. It is of the essence of voluntary effort that it is constantly evolving new forms. In most large towns within the last ten years Care Committees have been established, some merely to assist the Authorities in carrying out the more social powers and duties conferred on them by the Act; others with the higher ambition of 'building up the homes.' Such Care Committees have rendered a great service to their areas not only in work actually done under the direction of the Authority, but in the fact that they have frequently introduced new and opposite points of view from those of the administration. The Act of 1918 offers wider opportunities, and many social workers are beginning to realise it. During the last twelve months, in connection with the establishment of Day Continuation Schools, I have met in consultation, or addressed meetings, of social workers, trades-union representatives, club leaders, employers, clergy of various denominations, and parents; together and separately. I have met with opposition and criticism and divergent points of view, but what has gratified me most has been the general and eager desire for an increase in educational facilities and an improvement in social conditions. No subject for discussion has been so well received as that of training our young workers to use their leisure wisely. There has been the fullest recognition that all must join up in the common task; that the greatest opportunity of our time for joint endeavour in a wider educational effort has come; to miss it would be something in the nature of a betrayal of our several functions. If our continuation schools are to become national, not only in the sense of being universal and comprehensive, but in the generous nature of the spirit which inspires them, all that is best in our trade, social, and sports organisations must be brought to bear on their external and internal activities. On this ground alone I feel sure that there was general satisfaction that the guidance of the Juvenile Organisation Committees, and all that they stand for, was transferred from the Home Office, which has the great credit of having consolidated them, to the Board of Education, which is the official foster-mother of our educational system. In the London area the Juvenile Organisation Committees have gradually become representative in the widest sense of all social organisations, and it is anticipated that before long lines of co-operation with the Authority will be established. The task in all areas is so large that there is ample room for all; it is so complex that there is need for all and it is of such importance to the future that it would be a national misfortune not to welcome the service of all.

It is difficult for this generation to estimate with true insight the after-effects of the war. But it would seem as if there had rarely been a time when the minds of men were so much loosened from great principles. Such a condition is no doubt partly a reaction from a period of tense anxiety in which suppression of the individual and sacrifice for the community were the demands of a struggle for existence. But the general mental attitude may also be a reaction, accentuated by the

war, against the interpretation of the great principles which has hitherto directed us, to continue to deserve universal adherence. The outlook is yet clouded. Will the present individualistic point of view continue, or are we but being carried through a transition phase until the coming of a new rallying cry which will restate the brotherhood of man in some new and captivating form? However that may be our course seems clear: it is to develop the intelligence and the spirit of social service in our whole population in complete confidence that the solidity of the English character fortified with such weapons will maintain and expand that civilisation which has brought us so far, and which we owe it to posterity to hand on not only unimpaired, but broadened and deepened by new streams of thought and action. It is in this sense that the spread of educational advantages is the hope of all, and that I have made this appeal for all educational and social forces to concentrate in one national effort. In the words of one of our greatest poets:

Give all thou canst—  
 High Heaven rejects the lore  
 Of nicely calculated less or more.



# British Association for the Advancement of Science.

SECTION M: CARDIFF, 1920.

## ADDRESS TO THE AGRICULTURAL SECTION

BY

PROFESSOR FREDERICK KEEBLE, C.B.E., Sc.D., F.R.S.,

PRESIDENT OF THE SECTION.

### *Intensive Cultivation.*

THERE is, so far as I can discover, no reason—save one—why I should have been called upon to assume the presidency of the Agricultural Section of the British Association, or why I should have been temerarious enough to accept so high an honour and such a heavy load of responsibility. For upon the theme of Agriculture as commonly understood I could speak, were I to speak at all, but as a scribe and not as one in authority. The one reason, however, which must have directed the makers of presidents in their present choice is, I believe, so cogent that despite my otherwise unworthiness I dared not refuse the invitation. It is that, in appointing me, agriculturists desired to indicate the brotherhood which they feel with intensive cultivators. As properly proud sisters of an improved tale they have themselves issued an invitation to the Horticultural Cinderella to attend their party, and in conformity with present custom, which requires each lady to bring her partner, I am here as her friend.

Nor could any invitation give me greater pleasure: for my devotion to Horticulture is profound and my affection that of a lover. My only fear is lest I should weary my hosts with her praises, for in conformity with this interpretation I propose to devote my Address entirely to Horticulture—to speak of its performance during the war and of its immediate prospects.

Although that which intensive cultivators accomplished during the war is small in comparison with the great work performed by British agriculturists, yet nevertheless it is in itself by no means inconsiderable, and is, moreover, significant and deserves a brief record. That work may have turned and probably did turn the scale between scarcity and sufficiency; for, as I am informed, a difference of 10 per cent. in food supplies is enough to convert plenty into dearth. Seen from this standpoint the war-work accomplished by the professional horticulturist—the nurseryman, the florist, the glass-house cultivator, the fruit-grower and market gardener, and by the professional and amateur gardener and allotment holder assumes a real importance, albeit that the sum total of the acres they cultivated is but a fraction of the land which agriculturists put under the plough.

As a set-off against the relative smallness of the acreage brought during the war under intensive cultivation for food purposes, it is to be remembered that the yields per acre obtained by intensive cultivators are remarkably high. For example, skilled onion-growers compute their average yield at something less than 5 tons to the acre. A chrysanthemum-grower who turned his resources from the production of those flowers to that of onions obtained over an area of several acres a yield of 17 tons per acre. The average yield of potatoes under farm conditions in England and Wales is a little over 6 tons to the acre, whereas the army gardeners in France produced, from Scotch seed of Arrán Chief which was sent to them, crops of 14 tons to the acre. Needless to say, such a rate of yield as this is not remarkable when compared with that obtained by potato-growers in the Lothians or in Lincolnshire, but it is nevertheless noteworthy as an indication of what I think may be accepted as a fact, that the average yields from intensive cultivation are about double those achieved by extensive methods.

The reduction of the acreage under soft fruits—strawberries, raspberries, currants, and gooseberries—which took place during the war gives some measure of the sacrifices—partly voluntary, partly involuntary—made by fruit-growers to the cause of war-food production. The total area under soft fruits was 55,560 acres in 1913, by 1918 it had become 42,415, a decrease of 13,145 acres, or about 24 per cent. As would be expected, the reduction was greatest in the case of strawberries, the acreage of which fell from 21,692 in 1913 to 13,143 in 1918, a decrease of 8,549 acres, or about 40 per cent. It is unfortunate that bad causes often have best propagandas, for were the public made aware of such facts as these they would realise that the present high prices of soft fruits are of the nature of deferred premiums on war-risk insurances with respect to which the public claims were paid in advance and in full.

I should add that the large reduction of the strawberry acreage is a measure no less of the shortsightedness of officials than of the public spirit of fruit-growers; for in the earlier years of the war many counties issued compulsory orders requiring the grubbing up and restriction of planting of fruit, and I well remember that one of my first tasks as Controller of Horticulture was to intervene with the object of convincing the enthusiasts of corn production that, in war, some peace-time luxuries become necessities and that, to a sea-girt island beset by submarines, home-grown fruit most certainly falls into this category.

Those who were in positions of responsibility at that time will not readily forget the shifts to which they were put to secure and preserve supplies of any sorts of fruit which could be turned into jam—the collection of blackberries, the installation of pulping factories which Mr. Martin and I initiated, and the rushing of supplies of scarcely set jam to great towns, the populace of which, full of a steadfast fortitude in the face of military misfortune, was ominously losing its sweetness of disposition owing to the absence of jam and the dubiousness of the supply and quality of margarine.

But though the public lost in one direction it gained in another, and the reduction of the soft-fruit acreage meant—reckoned in terms of potatoes—an augmentation of supplies to the extent of over 100,000 tons. Equally notable was the contribution to food production made by the florists and nurserymen in response to our appeals. An indication of their effort is supplied by figures which, as president of the British Florists' Federation, Mr. George Munro—whose invaluable work for food production deserves public recognition—caused to be collected. They relate to the amount of food production undertaken by 100 leading florists and nurserymen. These men put 1,075 acres, out of a total of 1,775 acres used previously for flower-growing, to the purpose of food production, and they put 142 acres of glass out of a total of 218 acres to like use. I compute that their contribution amounted to considerably more than 12,000 tons of potatoes and 5,000 tons of tomatoes.

The market growers of Evesham and other districts famous for intensive cultivation also did their share by substituting for luxury crops, such as celery, those of greater food value, and even responded to our appeals to increase the acreage under that most chancy of crops—the onion, by laying down an additional 4,000 acres and thereby doubling a crop which more than any others supplies accessory food substances to the generality of the people.

In this connection the yields of potatoes secured by Germany and this country during the war period are worthy of scrutiny.

The pre-war averages were: Germany 42,450,000 tons, United Kingdom 6,950,000 tons; and the figures for 1914 were: Germany 41,850,000 tons, United Kingdom 7,476,000 tons.

Germany's supreme effort was made in 1915 with a yield of 49,570,000 tons, or about 17 per cent. above average. In that year our improvement was only half as good as that of Germany: our crop of 7,540,000 tons bettering our average by only 8 per cent. In 1916 weather played havoc with the crops in both countries, but Germany suffered most. The yield fell to 20,550,000 tons, a decrease of more than 50 per cent., whilst our yield was down to 5,469,000 tons, a falling off of only 20 per cent. In the following year Germany could produce no more than 39,500,000 tons, or a 90 per cent. crop, whereas the United Kingdom raised 8,604,000 tons, or about 24 per cent. better than the average. Finally, whereas with respect to the 1918 crop in Germany no figures are available, those for the United Kingdom indicate that the 1917 crop actually exceeded that of 1918.

There is much food for thought in these figures, but my immediate purpose in citing them is to claim that of the million and three-quarter tons increase in 1917 and 1918 a goodly proportion must be put to the credit of the intensive cultivator.

I regret that no statistics are available to illustrate the war-time food production by professional and amateur gardeners. That it was great I know, but how great I am unable to say. This however I can state, that from the day before the outbreak of hostilities, when, with the late Secretary of the Royal Horticultural Society, I started the intensive food-production campaign by urging publicly the autumn sowing of vegetables—a practice both then and now insufficiently followed—the



amateur and professional gardeners addressed themselves to the work of producing food with remarkable energy and success. No less remarkable and successful was the work of the old and new allotment holders, so much so indeed that at the time of the Armistice there were nearly a million and a-half allotment holders cultivating upwards of 125,000 acres of land: an allotment for every five households in England and Wales. It is a pathetic commentary on the Peace that Vienna should find itself obliged to do now what was done here during the war—namely, convert its parks and open spaces into allotments in order to supplement a meagre food supply.

This brief review of war-time intensive cultivation would be incomplete were it to contain no reference to intensive cultivation by the armies at home and abroad. From small beginnings, fostered by the distribution by the Royal Horticultural Society of supplies of vegetable seeds and plants to the troops in France, army cultivation assumed under the direction of Lord Harcourt's Army Agricultural Committee extraordinarily large dimensions: a bare summary must suffice here, but a full account may be found in the report presented by the Committee to the Houses of Parliament and published as a Parliamentary Paper.

In 1918 the armies at home cultivated 5,869 acres of vegetables. In the summer of that year the camp and other gardens of our armies in France were producing 100 tons of vegetables a day. These gardens yielded, in 1918, 14,000 tons of vegetables, worth, according to my estimate, a quarter of a million pounds sterling, but worth infinitely more if measured in terms of benefit to the health of the troops.

As the result of General Maude's initiative, the forces in Mesopotamia became great gardeners, and in 1918 produced 800 tons of vegetables, apart altogether from the large cultivations carried out by His Majesty's Forces in that wonderfully fertile land. In the same year the forces at Salonika had about 7,000 acres under agricultural and horticultural crops, and raised produce which effected a saving of over 50,000 shipping tons.

Even from this brief record it will, I believe, be conceded that intensive cultivation played a useful and significant part in the war: what, it may be asked, is the part which it is destined to play in the future? So far as I am able to learn, there exist in this country two schools of thought or opinion on the subject of the prospects of intensive cultivation, the optimistic and the pessimistic school. The former sees visions of large communities of small cultivators colonising the countryside of England, increasing and multiplying both production and themselves, a numerous, prosperous and happy people and a sure shield in time of war against the menace of submarines and starvation. Those on the other hand who take the pessimistic view, point to the many examples of smallholders who 'plough with pain their native lea and reap the labour of their hands' with remarkably small profit to themselves or to the community—smallholders like those in parts of Warwickshire, who can just manage by extremely hard labour to maintain themselves, or, like those in certain districts of Norfolk, who have let their holdings tumble down into corn and who

produce no more and indeed less to the acre than do the large farmers who are their neighbours.

Before making any attempt to estimate the worth of these rival opinions it may be observed that the war has brought a large reinforcement of strength to the rank of the optimists. A contrast of personal experiences illustrates this fact. When in the early days of the war I felt it my duty to consult certain important county officials with the object of securing their support for schemes of intensive food production, I carried away from the conference one conclusion only: that the counties of England were of two kinds, those which were already doing much and were unable therefore to do more, and those which were doing little because there was no more to be done. In spite of this close application of the doctrine of *Candide*—that all is for the best in the best of all possible worlds—I was able to set up some sort of county horticultural organisation, scrappy, amateurish, but enthusiastic, and the work done by that organisation was on the average good; so much so indeed that when after the Armistice I sought to build up a permanent county horticultural organisation I was met by a changed temper. The schemes which the staff of the Horticultural Division had elaborated as the result of experience during the war were received and adopted with a cordiality which I like to think was evoked no less by the excellence of the schemes themselves than by the promise of liberal financial assistance in their execution. Thus it came about that when the time arrived for me to hand over the controllership of Horticulture to my successor, almost every county had established a strong County Horticultural Committee, and the chief counties from the point of view of intensive cultivation had provided themselves with a staff competent to demonstrate not only to cottagers and allotment holders, but also to smallholders and commercial growers, the best methods of intensive cultivation. In the most important counties horticultural superintendents with knowledge of commercial fruit-growing were being appointed, and demonstration fruit and market-garden plots, designed on lines laid down by Captain Wellington and his expert assistants, were in course of establishment. The detailed plans for these links in a national chain of demonstration and trial plots have been published, and anyone who will study them will, I believe, recognise that they point the way to the successful development of a national system of intensive cultivation.

By means of these county stations the local cultivator may learn how to plant and maintain his fruit plantation and how to crop his vegetable quarters, what stock to run and what varieties to grow.

Farm stations—with the Research stations established previously by the Ministry; Long Ashton and East Malling for fruit investigations; the Lea Valley Growers' Association and Rothamstead for investigation of soil problems and pathology; the Imperial College of Science for research in plant physiology, together with a couple of stations, contemplated before the war, for local investigation of vegetable cultivation; an alliance with the Royal Horticultural Society's Research Station at Wisley, and with the John Innes Horticultural Institute for research in genetics; the Ormskirk Potato Trial Station; a Poultry

Institute; and, most important of all from the point of view of education, the establishment at Cambridge of a School of Horticulture—constitute a horticultural organisation which, if properly co-ordinated and—dare I say it?—directed, should prove of supreme value to all classes of intensive cultivators. To achieve that result, however, something more than a permissive attitude on the part of the Ministry is required, and in completing the design of it I had hoped also to remain a part of that organisation long enough to assist in securing its functioning as a living, plastic, resourceful, directive force—a horticultural cerebrum. Thus developed, it is my conviction that this instrument is capable of bringing Horticulture to a pitch of perfection undreamed of at the present time either in this country or elsewhere.

In my view Horticulture has suffered in the past because the fostering of it was only incidental to the work of the Ministry. In spite of the fact that it had not a little to be grateful for—as for example the research stations to which I have referred—Horticulture had been regarded rather as an agricultural side-show than as a thing in itself. My intention, in which I was encouraged by Lord Ernle, Lord Lee, and Sir Daniel Hall, was to peg out on behalf of Horticulture a large and valid claim and to work that claim. The conception of Horticulture which I entertained was that comprised in the ‘petite culture’ of the French. It included crops and stock, fruit and vegetables, flower and bulb and seed crops, potatoes, pigs and poultry and bees. I held the view, and still hold it, that the small man’s interests cannot be fostered by the big man’s care; that Horticulture is a thing in itself and requires constant consideration by horticulturists and not occasional help from agriculturally minded people, however distinguished and capable.

I had to include the pig and poultry, for the smallholder and commercial grower will have to keep the one and may with profit keep both, and he will have to modify his system of cultivation accordingly. The adoption of this conception of the scope of intensive cultivation opens up an array of new problems which require investigation, and it was my intention to endeavour to secure the experimental solution of these many problems at the Research Stations and elsewhere. Beside these problems—of green manuring, cropping, horticultural rotations—horticultural surveys would be made, ‘primeur’ lands demarked for colonisation, and existing orchard lands ascertained and classified, as indeed we had begun to do in the West of England. But, above all, with this measure of independence for Horticulture we, having the good will and support of the fraternity of horticulturists, aimed at putting to the test the certain belief which I hold that education—sympathetic and systematic—is an instrument the power of which, for our purpose, scarcely yet tried, is in fact of almost infinite potency. I believe with Mirabeau that, ‘after bread, education is the first need of the people,’ and I know that the people themselves are ready to receive it.

Contrast this horticultural prospect with the fact that a group of smallholders in an outlying district informed one of my inspectors that

his was the first visit that they had received for many years, or with the fact that remediable diseases are still rife in hundreds of gardens, or that few small growers understand the principles which should guide them in deciding whether or not to spray their potatoes, or that West Country orchardists exist who let dessert fruit tumble to the ground and sell it in ignorance of its true value, or that unthrifty fruit-trees may be top-grafted but are not, or that it is often ignored that arsenate of lead as a spray fluid for fruit pays over and over again for its use, or even that growers in plenty still do not know that Scotch or Irish or once-grown Lincolnshire seed potatoes are generally more profitable than is home-grown or local seed. The truth is that great skill and sure knowledge exist among small cultivators side by side with much ignorance and moderate practical ability. Herein lies the opportunity of the kind of education which I have in mind. But for any such intensive system of education to prevail the isolation both of cultivators and of Government Departments must be abolished. Out of that isolation hostility arises, in which medium no seed of education will germinate. It is troublesome, but not difficult, to abolish hostility. It vanishes when direct relations are established and maintained between a Department and those whose affairs it administers. The paternal method will not do it. The official life, lived 'remote, unfriendly, alone,' with only underlings as missionaries to the heathen public, will not do it.

There is only one way to prepare the ground for the intensive cultivation of education, and that is to secure the full co-operation of officials and cultivators. If this be not done the official must continue to bear with resignation the unconcealed hostility of those he wishes to assist. That a state of confidence and co-operation may be established is proved by the record of the Horticultural Advisory Committee which was set up by Lord Ernle during my controllership. The Committee consisted of representatives of all the many branches of Horticulture—fruit-growers, nurserymen, market gardeners, growers under glass, salesmen, researchers, and so forth. That Committee became, as it were, the Deputy-Controller of Horticulture. To it all large questions of policy were referred, and to its disinterested service Horticulture owes a great debt. That its existence has been rendered permanent by Lord Lee is of good augury for the future of intensive cultivation. As an instance of the judicial temper in which this Committee attended to its business I may mention that when an Order—the Silver Leaf Order—was under discussion the only objection to its terms on the part of the fruit-growers on the Committee was that the restrictive measures which it contemplated were not drastic enough: a noteworthy example of assent to a self-denying ordinance.

It may be asked What are the subjects in which growers require education? To answer that question fully would require an Address in itself. Among those subjects, however, mention may be made of a few: the extermination or top-grafting of unthrifty fruit, the proper spacing and pruning of fruit-trees, the use of suitable stocks, systematic orchard-spraying, the use of thrifty varieties of bush fruit and the proper manuring thereof, the choice of varieties suitable to given

soils and districts and for early cropping, the better grading and packing of fruit.

Of all methods of instruction in this last subject the best is that provided by Fruit Exhibitions. Those interested in the promotion of British fruit-growing will well remember the object-lesson in good and bad packing provided by the first Eastern Counties Fruit Show, held at Cambridge in 1919. That exhibition, organised by the East Anglian fruit-growers with the assistance of the Horticultural Division of the Ministry of Agriculture, demonstrated three things: first, that fruit of the finest quality is being grown in East Anglia; second, that this district may perhaps become the largest fruit-growing region in England; and, third, that among many growers profound ignorance exists with respect to the preparation of fruit for market.

The opinions which I have endeavoured to express on the organisation of intensive cultivation may be summarised thus:—

1. The object of the organisation is to improve local and general cultivation, the former by demonstration, the latter by research.

2. The method of organisation must provide for co-operation between the horticultural officers of the State and the persons engaged in the industry. This co-operation must be real and complete. Dummy Committees are silly devices adopted merely by second-rate men and merely clever administrators. The co-operation must embrace the policy as well as the practice of administration. Nevertheless the horticultural officers of the State must be leaders. They can, however, lead only by the power of knowledge. Wherefore an administrator who lacks practical knowledge and scientific training is not qualified to act as the executive head of a horticultural administration. The head must of course possess administrative capacity, but this form of ability is by no means uncommon among Britons, although it is a custom to represent it as something akin to inspiration and the attribute of the otherwise incompetent. The directing head must possess a wide practical knowledge of Horticulture; that alone can fire the train of his imagination to useful and great issues. His right-hand man, however, must be one versed in departmental and interdepartmental intricacies—the best type of administrator—of sober and cool judgment and keen intelligence, unused perhaps to enthusiasm, but not intolerant of nor immune from it. Similarly in each sub-department for cultivation, disease-prevention, small stock, &c., the head must be a trained practical man with an administrator as his chief assistant. The outdoor officers, the intelligence officers of the organisation, must also be men of sound and wide practical knowledge and must know that their reports will be read by someone who understands the subjects whereof they speak.

It was on these lines that the Horticultural Division was organised under Lord Ernle, Lord Lee, and Sir Daniel Hall. The work accomplished justified the innovation.

This is the contribution which I feel it my duty to make on the vexed question of the relation between expert and administrator in Departments of State which deal with technical and vital problems.

I believe that no administrator, save the rare genius, can direct the

expert, whereas the expert with trained scientific mind and possessed of a fair measure of administrative ability can direct any but a genius for administration. If the work of a Government office is to be and remain purely administrative no creative capacity is required, and it may be left in the sure and safe and able hands of the trained administrator; but if the work is to be creative it must be under the direction of minds turned as only research can turn them—in the direction of creativeness. To the technically initiated initiation is easy and attractive, to the uninitiated it is difficult and repugnant.

The useful work that such a staff as I have indicated would find to do is well-nigh endless. It would become a bureau of information in national horticulture, and the knowledge which it acquired would be of no less use to investigators than to the industry. Diseases ravage our orchards and gardens, some are known to be remediable and yet persist, others require immediate and vigorous team-wise investigation and yet continue to be investigated by solitary workers or single research institutions.

Certain new varieties of some soft fruits are known to be better than the older varieties, and yet the latter continue to be widely cultivated. The transport and distribution of perishable fruit is often inadequate—‘making a famine where abundance lies.’ The information gathered in during the constant survey of the progress of Horticulture would serve not only to direct educational effort into useful channels, but to stimulate and assist research. For the headquarters staff of trained men learns in the course of its administrative work many things, which, albeit unknown to the researcher, are of first importance to him who is bent on advancing horticultural knowledge.

For example, it is known that the trade of raisers of seed potatoes for export to Jersey or Spain is in some places menaced by the presence of a plot of land a mile or two away in which wart disease has appeared. It may be that the outbreak occurred on only a single plant, yet nevertheless the seed-potato grower may be inhibited from exporting the seed grown by him on clean land. The prohibition is just, but the man who refuses to issue a licence to export, if he be a trained horticulturist in touch with research, will know that there is research work to hand and that immediately, and will bring the problem to the urgent notice of the researchers. Thus the scientifically trained administrator becomes, although not himself witty in research, the cause of wit in others. To ask the researcher, who must inevitably be to some extent like Prospero ‘wrapt in secret studies and to the State grown stranger,’ to discover problems which arise out of administrative embarrassments is unreasonable; on the other hand, the scientifically trained administrator acts naturally as liaison officer between the laboratory and the land, passing on the problems which arise out of administrative necessities or expedients.

In this connection it is interesting to recall the fact that the importance of the existence of varieties of potatoes immune from wart disease was observed years ago by an officer of the Ministry, Mr. Gough, who is also a man possessed of a scientific training, and I believe also that I am right in saying that either this officer or another suggested

long ago that the clue to the spread of wart disease in England was to be sought in the potato fields of Scotland. Mr. Taylor will, I hope, give us the latest and most interesting chapter in the story of wart disease, and I will not therefore spoil his story by anticipation of its conclusions.

The tacit assumption which has so far underlain my Address is that an extension of intensive cultivation in this country is desirable. I have indicated that areas are to be discovered where soil and climate are favourable to this form of husbandry, and that by the establishment of a proper form of research—administrative—and educational organisation the already high standard reached by intensive cultivators may be surpassed. It remains to inquire whether any large increase in the area under intensive cultivation is in fact either desirable or probable.

The dispassionate inquirer will find his task by no means easy. He should, as a preliminary, endeavour to discern in the present welter of cosmic disturbance what are likely to be the economic conditions of the politician's promised land—the new world which was to be created from the travail of war. In the first place, and no matter how academic he may be, he cannot fail to recognise the fact that costs of production, including labour, are at least twice and probably  $2\frac{1}{2}$  times those of pre-war days, and he must assume that the increase is permanent and not unlikely to augment. What this means to the different forms of cultivation may be judged from the following estimates of capital costs of cultivation of different kinds:—

*Labour and Capital for Farming and Intensive Cultivation.*

—	Labour per 100 Acres	Capital per Acre	
		Pre-War	Present
	Men	£	£
Mixed Farming . . . . .	3-5	10	20-25
Fruit and Vegetable growing . . . . .	20-30	50	100-125
Intensive Cultivation in the open (French Gardening)	200	750	1,500-1,875
Cultivation under glass . . . . .	200-300	2,000	4,000-5,000

In the second place the inquirer is bound to assume that the intensive cultivator of the future, like his predecessor in the past, will have to be prepared to face the competition of the world. He may, I believe, look for no artificial restriction of imports, and therefore he must be prepared to find that higher costs of production will not necessarily be accompanied by increased receipts for intensively cultivated commodities.

But, on the other hand, he may find some comfort in the fact that both immediately before and, still more, subsequently to the war, the standard of living both in this country and throughout the world was, and is still, rising. Hence he may perhaps expect a less severe competition from foreign growers and also a better market at home.

He may also derive comfort from the reflection that the increased cost of production which he must bear must also, perhaps in no less

measure, be borne by his foreign competitors. Even before the war the cost of production of one of the chief horticultural crops—apples—was no higher in this country than in that of our main competitors. There are also certain other apparently minor but really important reasons for optimism with regard to the prospects of intensive cultivation. Among these is the increasing use of road in lieu of rail transport for the marketing of horticultural produce. The advantages of motor over rail transport for the carriage of perishable produce for relatively short distances—say up to 75 miles from market—lie in its greater punctuality, economy of handling, and elasticity. Only a poet native of a land of orchards could have written the lines: 'When I consider everything that grows holds in perfection but a single moment.' Fruit crops ripen rapidly and more or less simultaneously throughout a given district. They must be put on the market forthwith or are useless. A train service, no matter how well organised, does not seem able to cope with gluts, and hence it arises that a season of abundance in the country rarely means a like plenty to the consumer. I am aware that the problem of gluts is by no means simple and that the railways are sometimes blamed unjustly for failing to cope with them, but nevertheless I believe that, as Kent has discovered, the motor-lorry will be more and more called in to redress the balance between the home growers and the foreign producers in favour of the former; for by its use the goods can be delivered with certainty in time to catch the market and thus give the home producer the advantage due to propinquity which should be his. Increasing knowledge of food values, together with the general rise in the standard of living, also present features of good augury to the intensive cultivator. Jam and tomatoes and primeurs may be taken as texts.

In 1914 the consumption of jam in the United Kingdom amounted to about a spoonful a day per person. The more exact figures are 2 oz. per week, or 126,000 tons per annum.

It is difficult to estimate the area under jam fruit—plums, strawberry, raspberry, currants, &c.—required to produce this tonnage, but it may be put at between 10,000 and 20,000 acres.

By 1918, thanks to the wisdom of the Army authorities in insisting on a large ration of jam for the troops, and thanks also to the scarcity and quality of margarine, the consumption of jam had more than doubled. From 126,000 tons of 1914 it reached 340,000 tons in 1918. To supply this ration would require the produce of from 25,000 to 50,000 acres of orchard, which in turn would directly employ the labour of say from 5,000 to 10,000 men. Yet even the tonnage consumed in 1918 only allows a meagre ration of little more than a couple of spoonfuls a day. It may therefore be anticipated that if, as is probable, albeit only because of the immanence of margarine, the new-found public taste for jam endures, fruit-growers in this country will find a considerable and profitable extension in supplying this demand.

The remarkable increase in consumption which the tomato has achieved would seem to support this conclusion. Fifty years ago, as Mr. Robbins has mentioned in his paper on 'Intensive Cultivation' (Journal of Board of Agriculture, xxv. No. 12, March 1919), this



fruit was all but unused as a food. To-day one district alone, the Lea Valley, produces 30,000 tons per annum. The total production in this country amounts to upwards of 45,000 tons. Yet the demand for tomatos has increased so rapidly—the appetite growing by what it feeds upon—that the imports in 1913 from the Channel Islands, Holland, France, Portugal, Spain, Canary Islands, and Italy amounted to nearly double the home crop, viz. 80,000 tons, making the total annual consumption not less than  $1\frac{1}{4}$  tons or about 2 pounds per week per head of population. Is it too fanciful to discern in this rapidly growing increase in the consumption of such accessory foodstuffs as jam and tomatos, not merely an indication of a general rise in the standard of living and a desire on the part of the community as a whole to share in the luxuries of the rich, but also a sign that in a practical, instinctive, unconscious way the public has discovered simultaneously with the physiologists that a monotonous diet means malnutrition, and that even in a dietic sense man cannot live by bread alone? As lending support to this fancy and as indicating that the value of vitamines was discovered by people before vitamines were discovered by physiologists, I may mention the curious fact that the general public has always shown a wise greediness for an accessory food which, though relatively poor in calories is rich in vitamines—namely the onion. Even in pre-war times the annual value of imported onions amounted to well over one million pounds sterling; and, when the poverty of the winter diet of the people of England and Wales is considered, it must be admitted that this expenditure represented a sound investment on the part of the British public. It is a curious fact also that the genius of Nelson led him to a like conclusion. He took care, during the long years when his blockading fleet kept the seas, to provide his sailors with plenty of exercise and onions.

If, as I think, the increasing consumption of the accessory foods which intensive cultivation provides represents not merely a craving for luxuries, but an instinctive demand for the so-called accessory food bodies which are essential to health, then it may be expected that, as has been illustrated in the case of jam and potatos, consumption will continue to increase. If this be so, the demand both for fresh fruit and also for 'primeurs'—early vegetables—should grow and should be supplied at least in part by the intensive cultivators of this country.

If the home producer can place his wares on the market at a price that can compete with imported produce—and it is not improbable that he will be able to do so—he need not, even with increased production, apprehend more loss from lack of demand than he has had to face in the past. Seasonal and other occasional gluts he must, of course, expect.

Even when judged by pre-war values, his market, as indicated by imports, is a capacious one. Thus in 1913 the imports into the United Kingdom of soil products from smallholdings were of the value of about 50 million pounds sterling. To-day it is safe to compute them at over 100 millions. To that sum—of 50 millions—imported vegetables contributed  $5\frac{1}{2}$  million pounds sterling, apples  $2\frac{1}{4}$  millions, other fruits nearly 3 millions, eggs and poultry over 10 millions, rabbits and rabbit-

skins a million and a half, and bacon and pork over 22 millions. No one whose enthusiasm did not altogether outrun both his discretion and knowledge would suggest that the home producer could supply the whole or even the greater part of these commodities. But, on the other hand, few of those who have knowledge of the skill and resources of our intensive cultivators, and of the suitability of favoured parts of this country for intensive cultivation, will doubt but that a modest proportion, say, for example, one fifth, might be made at home. This on a post-war basis would amount in value to over 20 million pounds, would require the use of several hundred thousand acres of land and provide employment for something like 100,000 men. The fact that Kent has found it profitable to bring one-fifth of its total arable land under fruit and other forms of intensive cultivation is significant and a further indication that intensive cultivation offers real prospects to the skilful and industrious husbandman. The present reduced acreage under fruit, due partly to war conditions, but mainly to the grubbing of old orchards, enhances the prospects of success.

The estimated acreage under fruit in England and Wales is:—

	Acres
Apples . . . . .	170,000
Pears . . . . .	10,000
Plums . . . . .	17,000
Cherries . . . . .	10,000
Strawberries . . . . .	13,000
Raspberries . . . . .	6,000
Currants and Gooseberries . . . . .	22,000
	<hr/> 248,000

exclusive of mixed orchards and plantations.

These figures are, however, well-nigh useless as indicating the areas devoted to the intensive cultivation of fruit for direct consumption. Of the 170,000 acres of apples, cider fruit probably occupies not less than 100,000, and of this area much ground is cumbered with old and neglected trees. Of the 10,000 acres in pears some 8,000 are devoted to perry production, and hence lie outside our immediate preoccupation. Having regard, however, to the reduction of acreage under fruit, to the increasing consumption of fruit and jam, and to the success which has attended intelligent planting in the past, it may be concluded that a good many thousand acres of fruit might be planted in this country with good prospects of success.

Lastly, it remains to consider what results are likely to occur if intensive cultivation comes to be more generally practised in this country. I am indebted to one of our leading growers for an example of the results which have attended the conversion of an ordinary farm into an intensively cultivated holding.

The farm—of 150 acres and nearly all arable—was taken over in 1881. At that date it found regular employment for three men and a boy—with the usual extra help at harvest. The rate of wages paid to the farm hand was 15s. a week.

In 1883, two years after the farm had been taken over and converted

to the uses of a horticultural holding, from 20 to 25 men and 80 to 100 women, according to season, were at work on it, and the minimum wage for men was 20s. per week. The holding was increased gradually to 310 acres, and at the present time gives employment on an average to 90 men and 50 women during the winter months and 110 men and 200 women during the summer months. In 1913 the wages bill was 7,981*l.*, and in 1918 10,000*l.* per annum, that is, over 34*l.* per acre.

Another concrete example of the effect of intensity of cultivation on density of population is provided by the comparison of two not far distant districts—Rutland and the Isle of Ely. The rich soil and industrious temperament of the inhabitants of the Isle have justly brought it prosperity and fame. The Isle of Ely comprises 236,961 acres, of which number 170,395 are arable; Rutland 97,087 acres with 35,000 arable. The land of Rutland is occupied by 475 persons, that of the Isle by 2,002; the average acreage per occupier in Rutland is 206, in the Isle 118. The total number of agricultural workers in Rutland is 2,146, and in the Isle 13,382. The density of agricultural population in terms of total acreage is in Rutland 2.5 per 100 acres, and in the Isle 5.6, or 20 more cultivators to the square mile in the Isle of Ely than in Rutland; from which the curious may estimate the possibility of home colonisation by introducing as a supplement to extensive agriculture such an amount of intensive cultivation as may be practised in districts similar in climate and soil to the Isle.

The immediate object of the comparison is to show, however, that the difference between the closeness of colonisation of the two lands is accurately presented by the difference between the acreages amenable to intensive cultivation which by reason of soil must, however, always remain relatively larger in the Isle than in Rutland. Thus in Rutland the area under fruit is 204 acres, and in the Isle 7,126. If these areas and the workers thereon be deducted from the total arable in the two districts, the respective agricultural populations in terms of 100 acres of arable become almost identical, viz. 6.7 for Rutland and 6.9 for the Isle. The difference of agricultural populations is measured by the area under intensive cultivation. The agricultural workers engaged on the 7,126 acres of fruit in the Isle of Ely are almost as numerous as those engaged in doing all the agricultural work of Rutland—say, about 2,000 as compared with 2,416.

It may of course be true that a chance word, a common soldier, a girl at the door of an inn, have changed, or almost changed, the fate of nations, but it is probable that the genius of peoples and the pressure of economic and social forces are more potent. Is there then, it may be asked, any indication that the people of this country will seek in intensive cultivation a means of colonising their own land rather than continue to export their surplus man-power? The problem is too complex and too subtle for me to solve, but I will conclude by citing a curious fact which may have real significance in indicating that if a nation so wills it may retain its surplus population on the land by adjusting the intensity of its cultivation to the density of its population. If a diagram be made combining the intensity of pro-

duction of a given crop, *e.g.*, the potato, as grown in the chief industrial countries of the world, it will be found that the curve of production coincides closely with that of density of population.

*Density of Population and Intensity of Production. Potatoes.*

	Density of Population Square Mile.	Percentage of Population.	Percentage of Yield.	Yield in Tons per acre less seed. Average 1911-13.
United States . . .	31	10	33	1·3
France . . .	193	62	56	2·2
Germany . . .	311	100	100	3·9
U.K. . . . .	374	120	110	4·3
England and Wales .	550	177	128	5
Belgium . . .	658	212	155	6·04

From these facts we may take comfort, for they indicate that as a population increases so does the intensity of its cultivation: the tide which flows into the towns may be made to ebb again into the country. The rate of return, however, must depend on many factors: the proclivities of peoples, the relative attractiveness of urban and rural life and of life at home and abroad, but ultimately the settlement or non-settlement of the countryside must be determined by the degree of success of the average intensive cultivator. The abler man can command success; whether the man of average ability and industry can achieve it, will, I believe, depend ultimately on education. He can look for no assistance in the form of restricted imports. He must be prepared to face open competition. Wherefore he should receive all the help which the State can render; and the measure of success which he, and hence the State, achieves will be determined ultimately by the quality and kind of education which he is able to obtain.